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ART. I.—MISCELLANEOUS CASES.

BY JOHN C. DARBY, M. D., OF LEXINGTON, KY.

In the *Medical Examiner and Record of Medical Science*, for July, 1852, I find a notice of the following case in the *London Lancet:* "*London Hospital.*—Abscess of the Lung following Pneumonia; death; autopsy; under the care of Dr. PEREIRA;" with these remarks:—"The formation of abscess in the lung as a consequence of pneumonia is considered a very rare occurrence, but it is probably still more rare to find a circumscribed accumulation of pus in the pulmonary tissue taking on a semi-gangrenous character." Having recently had such a case, and as it seems to be so rare, I report it for the *Western Journal of Medicine.*

On the 17th of September, 1851, I was called to see Mr. JAMES R. BOLTON, blacksmith, some 23 or 24 years of age. His disease was typhoid fever of a very grave form. He was at the house of two poor widows, who had kindly taken him in when he was taken sick, and who informed me that he had been sick for near a week, and had been out of his head from the first. He had had diarrhœa from the first, and would several times during the day dress himself, say he was well and was going down town. He would go down stairs, and by the time he got there he was unable to go any farther. I found him in this condition, with the same disposition to go out of doors.

It is unnecessary to state the treatment for the typhoid fever. I had him removed to the house of a friend who was better able to attend to him, and in about three weeks he was convalescent. He seemed to be doing well for near a week, when he was suddenly attacked with pneumonia of the right lung. For the pneumonia I gave him the following mixture, (which by the way I recommend as a remedy that will suit as many cases of pneumonia after the first onset as any other formulary I have ever tried)—viz:

Brandy and water, made of usual strength,	℥viii.
Tartar. antimony,	grs.v.
Laudanum	℥. mix.

The dose of this mixture is ℥ss. The quantity of tartar emetic may be increased or lessened. p. r. n.

He took a dose of this mixture every two hours, if not asleep. The pneumonia commenced about the 4th of October, by the 11th or 12th that disease seemed to be subdued, except that there was still an occasional pain in the side. He continued to improve for four or five days longer, when there commenced a very fœtid odor whenever he coughed. It smelled like suphuretted hydrogen, and the expectoration was purulent, and copious. I then decided that it had become a case of abscess of the lung. There was dullness on percussion over the right side, and the respiratory murmur was very indistinct. All that I did after this was to endeavor to sustain his strength. I gave nutritious food, brandy and cod-liver oil. He died on the night of the 25th of October. From the time that this fœtid odor first commenced up to the hour of his death, it continued, and his nurses were frequently compelled to leave the room on account of it. He was very much emaciated. His appetite had continued to be quite good from the time that he seemed to be well of typhoid fever. I considered him to be a young man of ordinarily good constitution.

Autopsy.—Drs. B. P. DRAKE, J. M. BRUCE, and W. CROMWELL assisted in the post mortem examination. There was nothing remarkable except in the right lung, in the central portion of which there was a circumscribed abscess containing near a pint of fœtid pus. The abscess had opened and discharged into the cavity of the chest. There was a large amount of pus about the lung on the right side.

Remarks.—My opinion is that the young man would have recovered from the typhoid fever and got well, had he not been seized with pneumonia; and that the latter degenerated into pulmonary abscess in consequence of the enfeebled state of his system consesequent upon his protracted and severe illness.

CASE 2.—Pneumonia of the left lung with solidification of that organ.

I was called to see Mr. N. B. E——. on Sunday evening, May 13, 1849. He had been treated for pneumonia for near a week, by two of the best physicians of the city. Before going to see the patient (who was a pauper case) I called on one of them, who informed me that I was at perfect liberty to take charge of the case, and that he regretted that he did not tell the family in the morning that he should not return, as he considered the case hopeless, that the left lung was completely solidified, &c., &c. I found the left lung to be as stated by Dr. ——. It was perfectly dull on percussion, and entirely impervious to the air. He also had had hiccups for twenty-four hours or longer, off and on, and it was very severe when I saw him. The pulse, though not very strong or full, yet as I thought, justified bleeding. The patient's voice was pretty good, and he seemed to have no idea of dying. His bowels were constipated. I bled him fully a quart, with very great relief. I then gave him twenty grains of calomel, and two hours afterwards gave him ten grains of musk. The musk was repeated every four or six hours according to the severity of the hiccups. By the next morning he was much better, but the hiccup continued for three days. During this time I gave twenty grains of calomel night and morning, and the musk as above. The patient also had good rich broths, and brandy milk punch. After the hiccup subsided and he began to rally I used tartar emetic in small quantities mixed with brandy and water, and laudanum as directed in the first case. I dismissed this patient on the 12th of June. But in the interval he had several returns of the hiccup and had several times appeared to be on the verge of the grave. He so far recovered his health as to give his usual weight, and to look as well as ever. He was able to attend to his business, and did well until the summer of 1851, when he began to be troubled

with a bad cough. This turned out to be a rapid consumption, and he died early in October, 1851.

Accidentally, I was prevented from making a post mortem examination, which I very much regretted. I oftentimes examined this man after his first convalescence, but could never hear any respiratory murmur in the left lung, except high up near the clavicle.

Remarks.—Physicians should not despair too soon of a case of pneumonia when one lung is sound. Calomel in decided doses is one of the most valuable remedies in this disease when the bowels are not too much acted on; and when they are if you will give tartar emetic in combination with brandy and laudanum, as above directed, you may still give the calomel with great advantage. Of all the remedies for pneumonia, judiciously used, bleeding has perhaps saved as many lives in desperate cases as any other remedy. My opinion is that the bleeding, calomel and musk saved this man's life in the first instance, and that good treatment afterwards restored him to health and prolonged his life.

CASE 3.—Obstetrics.

I was called to see Mrs. I——., aged twenty-eight years, on the night of July 5, 1852; there were slight labor pains—presentation natural. The patient continued in this condition until the morning of the 7th, when the pains became more severe. Nothing unusual occurred, and the lady was delivered of a fine hearty boy weighing about seven pounds, a few minutes after 5 o'clock in the afternoon. In removing the placenta I observed that there was a separate body of small size attached to it. When the placenta had all come away I examined this body, and easily tore it open with my fingers. I found within a fœtus about six inches long, perfectly developed as to the limbs and body, the head was about the size of a Spanish dollar and not much thicker, *i. e.* it was flat. I gave the dead fœtus to Prof. LETCHER of Transylvania Medical College to show to the class. There was nothing unusual in this lady's case. She had been married several years. She says that she thinks that constant sewing was the cause of the death of the second child. The mother and son are, to-day, (July 15) doing very well.

In the Louisville *Daily Democrat*, for the 15th of Sept., I find a notice of "a Boston broker, Mr. George H. Goodin," having cut the tendo-Achilles with an adze, with this remark: "The physician states that the severed cords will never unite, and he must submit to be a cripple for life." This is a mere newspaper notice, and may not be entitled to any attention; but as I have treated a far worse case successfully, I send you a short notice of it for your medical journal.

CASE 4.—On the 10th of October, 1852, I was called to see John Ackman, who had been employed to pull down a brick wall. Finding that a part of the wall was about to fall he turned in haste to make his escape, when the leg of his pantaloons at the ankle was caught by a nail and held him, and, as he says, about three hundred bricks fell on his leg. Both bones were broken about two and a half or three inches above the ankle. The tendo-achilles, for its entire length, was separated into strings not larger than a fiddle-string. The broken bones were adjusted, and a bandage applied from the ends of the toes to near the knee. Cold water dressings were then constantly applied, off and on, as necessary, until the patient recovered. The plan was to wrap carefully and gently over the bandage and around the wounded parts soft cloths wrung out of cold water, and then to put a dry cloth over the wet one, and to renew the wet cloth as often as it got too hot or began to dry. The broken bones united without difficulty, but the tendo-achilles sloughed for three or four inches, nevertheless a new cartilage was formed and on the 25th of December following I dismissed him cured. The parts were tender for some time, but he soon went to work as a day laborer and has ever since done the heaviest work, as lifting bags in a mill, &c., &c. To see him walk, no one would ever suspect that he had ever been injured, and to examine the leg you could hardly believe that the tendo-achilles was ever destroyed.

This case proves two things:—1st. The wonderful recuperative powers of nature. 2d. The great value of the bandage when properly applied; and 3d., I may add, the great virtue of cold water dressings.

Sept. 1854.

Art. II.—EXTRACTS FROM AN UNPUBLISHED REPORT ON THE MEDICAL LITERATURE OF KENTUCKY.

BY LUNSFORD P. YANDELL, M. D., &C.

Dr. Daniel Drake, though a citizen of Ohio at the time of his death, spent a large portion of his active life in Kentucky, and is justly claimed by her as one of her Medical Authors. Having returned to his practice in Cincinnati, in 1818, at the breaking up of the Medical Faculty of Transylvania University of which he was a member, and having engaged in an unsuccessful effort to rear a Medical School in that city, he was induced to accept again the chair in Lexington, which he had resigned five years before. His first connection with this school gave rise to nothing of a public professional character that merits a notice in this Report, although previous to this time Dr. Drake had become an author of note. His "Picture of Cincinnati" had made him known to scientific men abroad, and he returned to Transylvania University with a high reputation for ability and learning. The public Lecture which he delivered introductory to his first course on Materia Medica, November, 1823, was published at the request of the Medical Class. The subject of the lecture was "The Incentives to professional study," and his manner of treating it was esteemed peculiarly happy. It was an eloquent discourse, abounding in thoughts calculated to excite the ambition and kindle the zeal of his pupils.

This inaugural address, we believe, is the only publication of which Dr. Drake's reunion with the Lexington Medical Faculty was productive. He resigned his professorship in Transylvania a second time, in 1827, at the close of his fourth course of lectures. In 1839 he was invited to Louisville, to a chair in the Medical Institute, just then entering upon its third session, and we find him in the winter of that year lecturing on Pathological Anatomy and Clinical medicine, and contributing again to the Medical lit-

erature of Kentucky. His Valedictory lecture to the graduating class was published. The subject of the lecture was the importance of clinical instruction to students of medicine. It was determined by the Faculty soon after Dr. DRAKE came into the school, to revive the medical journal which had been suspended after the issue of the second number, and he consented to become one of the editors. The first number of the *Western Journal of Medicine and Surgery* was published on the first of January, 1840. The name is nearly the same as that of the Journal which he had conducted in Cincinnati to the tenth volume, and of which he regarded this as a continuation. The pages of the first number were enlivened by some characteristic editorial articles from his pen, in which light, off-hand work, where the pun, the anecdote, wit, and humor are blended agreeably with science, he greatly excelled. During his connection with the journal he indulged freely his taste for such writing. His "Traveling Editorials," as he termed them, letters written to his colleagues during his tours to collect the materials of his great work since published, were read, perhaps, with as much interest as any other articles in the journal. He reported in the second volume (p. 343), a case of "Intermittent Fever, complicated with Hepatitis, Pneumonitis, Laryngitis, and Intestinal irritation." He closes his history of t case with the following characteristic paragraph:

"To such of our readers as may think their time lost by the perusal of this article, we beg leave to say, that the afternoon (July 29), is oppressively hot; and that, if we were writing in as pleasant weather as they will read, we should no doubt forestall them in that criticism."

At the Medical Convention held at Frankfort, in January, 1841, he read an elaborate memoir on "the Disease called by the people 'Trembles,' and 'Sick stomach,' 'Milksickness,' as they occurred in the counties of Fayette, Madison, Clark, and Green, Ohio." He proceeds first to show by "way of exclusion," what agents cannot give rise to the disease, and arrives, by this process, at the conclusion that it is probably engendered by the *Rhus radican*. In his first object he is entirely successful; he proves most satisfactorily that milksickness cannot originate in any of the causes that have been heretofore assigned; but in the second he seems to us to have failed as signally as any who had written on

the subject before him. His memoir, however, is a model of such investigations, and will be read with interest by those who are curious about the question of which it treats.*

The same Journal (vol. 6, p. 401), contains an interesting paper by Dr. Drake, the title of which is, "The Northern Lakes, a Summer resort for Invalids of the South." Its suggestions are judicious and important. It is written with the freshness of a traveler who had beheld the charming scenery he describes, and with the sagacity and judgment of an able, experienced physician.

In 1843 Dr. Drake engaged with a number of friends in the investigation of the phenomena of Mesmerism, which, at that time, was exciting great interest in Louisville. The point to which his attention was particularly directed was *clairvoyance*. In order to test the reality of this alleged faculty, he arranged that several gentlemen should write down a series of topics to be presented to the "subject" when in the mesmeric state, by reference to which, the answers being recorded, it would be seen whether the person mesmerised had before his mind the objects in the mind of the individual "in relation" with him. The test was fair, and at the same time decisive. The result was a conviction on the part of the majority of the committee, that there was no remarkable coincidence between the answers of the "subject" and the objects in the mind of the questioner. Dr. Drake published a detailed account of the inquiry under the head of "Analytical Report of a Series of Experiments in Mesmeric Somnambulism.† It will, perhaps, strike the reader as tedious, but it is conclusive of the question. The experiments proved that all was guess beyond what was suggested to the mind of the "subject" by the individual asking the questions. This Dr. Drake exhibited most clearly in a subsequent paper on the subject, entitled "Speculations on the Facts presented in the Analytical Report on Mesmeric Somnambulism." He shows in this article that a large proportion of the answers returned were not correct, that many were vague, and that those which seemed to favor the idea of clairvoyance were suggested by the questions proposed. We

*See Western Journal Med. and Surg., vol. iii.

†Western Jour. Med. and Surg. (New Series), vol i, p. 189.

have rarely read a paper displaying greater mental acumen than this, and we could not point to one which exhibits more of the peculiar traits of the author's mind. It will be found to contain the germ of the true theory of all those phenomena occurring in the conditions variously styled "clairvoyance," "biology," "hypnotism," &c. The explanation is, briefly, that the mind of the individual in the mesmeric state is in a peculiarly ductile condition, and may be directed, at will, by the person who may chance to have his ear when he passes into this state; or, as explained by Dr. CARPENTER, he is a "thinking automaton," having no will of his own, but being completely under the guidance of the person in relation with him. Whatever the questioner sees, a word will cause the "subject" to see. But we need not go into a fuller explanation. We have said this much because at the time when Dr. DRAKE submitted his "Speculations" to the profession, the subject was very imperfectly understood, and he has the credit of having discovered the clue to phenomena which, to many highly cultivated minds, appeared mysterious and wonderful.

In 1850 appeared his great work on the Principal Diseases of the Interior Valley of North America,* a work which he had meditated for forty years. As long ago as 1823 he issued a printed circular to the physicians of the South-West, announcing his purpose to write a work on the diseases of the Ohio Valley, and soliciting the contribution of results afforded by their experience and observation. Engaged as he has been since in the labors of practice, and engrossed by the politics of medical schools, he has never lost sight of this noble enterprise. Amid all the changes through which he has passed in the Medical Colleges of three States, his plans in regard to this great labor of his life have remained unaltered. The institution founded by his exertions in early life in the city which he loved, the image of which has pursued him in all his subsequent journeyings, and made him restless and dissatisfied in every other institution with which he has been connected—an image beckoning him still to return to his

*A Systematic Treatise, Historical, Etiological and Practical, on the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian and Esquimaux varieties of its population.

early home, and disappointing him still as often as he yielded to its importunity--has never been able to divert his thoughts from this cherished object. It shows great steadiness of purpose that, amid every distracting care and throughout the unquiet life of the most active of medical politicians, the plan of his book was kept constantly before his mind. It is fortunate for his fame that these exciting concerns never wholly engrossed his thoughts.

About nine years before his work appeared he set earnestly about collecting materials for its composition, by exploring personally the great region of which it was to treat. Connected at the time with the Medical Institute of Louisville, and not disposed any longer to engage actively in the practice of physic, he enjoyed in the long vacations of the school the leisure necessary to the extended journeys required by the arduous enterprise. In the spring of 1841 he commenced a series of excursions in which he visited a wide area of country on the Mississippi, Ohio, Alabama, Tennessee, Missouri and St. Lawrence rivers, from the Balize to the Indian territory in Missouri, and from Pensacola Bay to Lake Superior. In the course of his travels he sought out the medical men everywhere, and collected their observations respecting the diseases of their several countries. It was his purpose to see the Great Valley in all its aspects, and its population in all its varieties and grades of civilization. He saw men in Barracks and in Indian wigwams, and made observations on the Caucasian, the African, the Indian, and the Esquimaux. And in addition to all this, he has availed himself of the observations of other travelers in remote parts of the Valley not explored by himself, thus embracing in his treatise the medical topography of the country from Vera Cruz to the regions around Hudson Bay.

It was to produce a work on the diseases of the Interior Valley out of materials collected in the region where they prevailed, that Dr. Drake was making preparation through this long period and by this extended travel. He never thought of framing a treatise out of other books. He meant that his work should be indigenous. No blind advocate of sectional medicine, he had yet observed enough to convince him that diseases were modified, to some extent, by the countries in which they prevailed. "That many physicians," says he, "overrate the degree of variation from a

common standard which the diseases of different countries present, I am quite convinced; but feel equally assured that if the maladies of each country are studied and described without reference to those of any other, it would be found, if the state of medical science were equal in them, that the works thus produced would not be commutable, but that each would be better adapted, as a book of etiology, diagnosis and practice, to the profession and people among which it was written, than to any other."* It was his design to ascertain how far the diseases of the interior Valley differed from the same affections as described by writers elsewhere, and to make his work faithfully reflect these diversities.

The causes that go to modify disease are soil, climate, and population. Dr. DRAKE discusses these subjects at great length, four-fifths of his great volume being occupied with their consideration. Many have expressed regret that he should have given such prominence to etiology in a work designed for practical men; but to have given to it less space, or to have detached it from his treatise on the diseases of the West, as many would prefer that he should have done, would have been to abandon one of his most cherished ideas. He believed that the range of professional studies in our country was too limited, and it was his purpose to show, in this work, the full bearing of etiology upon practical medicine. He has presented the most extended system of medical topography to be found in any work extant. In accumulating and digesting this huge mass of details he was well aware, that, for the majority of medical men, he was performing a profitless and thankless labor. He foresaw that by the many his design would not be understood. But it was his theory of medical science, and no fear that his book would be less popular interfered with his putting it into practice.

As among the elements of Etiology, he begins with the mineral geology of the various regions described, holding that this is the proper basis of medical topography. It is well understood that the character of surface influences materially the health of a country, but no writer has indicated so clearly the relations of geological to medical science as Dr. DRAKE has done in this treatise.

He devotes two hundred pages of his work to the climtae of this

* Introd. Interior Valley.

great intramontane plane. And finally he presents a comprehensive view of its hygiene, or physiological and social etiology. He treats the subject not in a general way, but with special reference to our own population, the facts and conclusions having been derived from the author's own observation. As the climate of every country possesses something peculiar, so there are peculiarities in the hygienic condition of every people; and in one element at least the population of the great Valley stands more conspicuously than any other people upon earth; for where else, as Dr. Drake has pointed out, was there ever before such a commingling of races? Four out of five of the grand varieties of the human family are here—the Caucasian, African, Indian and Mongolian; and we have every branch of that family which stands first among the races—English, Irish, French, Italian, German, Scotch, Spaniard, Pole, Norwegian, Swiss, Swede, Hollander, Welch, and Jew, united here to form one great Caucasian family. For a century they have been mingling with each other and with the aborigines of the country, and the process of amalgamation which has been going on among men from the earliest ages, as shown by Dr. Drake, must go on in an accelerated ratio and here attain its final consummation.

"The great Central Valley of North America is the best crucible into which living materials, in great and diversified streams, can be poured for amalgamation. The double range of mountains which separate it from the Pacific Ocean, leave too little space for empire on that sea; and the detached communities which may there grow up, will be but deviations from the homogeneous millions, with which time will people the great region of the Appalachian and Rocky mountains, which is thus destined to present the last and greatest development of society."

Such are the broad, philosophical views in which this treatise of Dr. Drake abounds. This work is no compilation; it is no mere technical treatise on disease, but a scientific account of the characteristics of the great West, geological, ethnological, sanitary and climatorial.

The gifted author had the second volume of his treatise nearly ready for the press when summoned away from the scene of his earthly labors. His last published work was a small volume consisting of two Discourses, delivered before the Cincinnati Medical

Library Association, in January 1852. In the first of these he gives a biographical sketch of the early physicians of Cincinnati, with some incidents in his own eventful life; the subject of the second discourse is "Medical Journals and Libraries," of the rise of which he presents an interesting history.

The style of the first is light and sketchy, adapted to the simple annals which it was the purpose of the discourse to record. The second discourse abounds in fine reflections. In the following remarks, so full of the true spirit of philosophy and glowing with poetical beauty, the author might be supposed to be giving a description of himself:

"The enlightened aspirant for medical distinction is but little taken with the applause of the present moment, unless he perceives that after it has passed away there will remain some enduring element of fame—some solid and undecaying trunk, with fruit-bearing branches, which might have been overspread and hidden by a gorgeous drapery of leaves and flowers that enraptured the gazing populace for a day, but had no charms for him. When we feel in our hearts this indifference to the fleeting, and this warm regard for the permanent, let us believe that God has implanted the instinct for a wise and good purpose; and then follow it as a heavenly guide."

And again—in speaking of that class of medical works to which his own treatise confessedly belongs, he says:

"The true and healthful bibliothecal element of the mind consists of monographs, reports of original observations, and the systematic works which, in different ages, have presented the existing principles or acts of the profession, as they appeared to men of genius and experience. Such works have been at all times appearing and disappearing. They are not, like ordinary compilations, superseded by others of a later imprint, but a new generation overlooks them. They are like great men who have retired from the public gaze. They lie embalmed in the truths with which they are penetrated, and cannot decay; but like the bodies of kings and philosophers, steeped in spices and deposited in catacombs, they are found only in the unfrequented closets and alcoves."

No one, we think, who will look patiently through the writings of Dr. Drake, will fail to come to the conclusion, that they are penetrated by truths which will preserve them from decay, and cause them to be referred to by the medical philosopher and his-

torian of future times. Our belief is, that few medical writers in our country have contributed to the profession a greater number of original observations, with a smaller admixture of error. In a diligent search through all his numerous publications it would not be easy to find one hasty, much less absurd, opinion, a single groundless hypothesis, or an idle speculation. He was a cautious writer. He was slow to avow his opinions. In nothing was he ultra. As a reviewer and critic he failed, because he was afraid to use the critical power which he possessed. He seemed reluctant to condemn or contradict. He speculated but little, and was never so much in his proper element as when putting forth his own observations. He loved research and phenomena. He was satisfied with nothing but facts. His mind was acute, analyitical, and logical, and if he had given himself up to it, he might have made one of the keenest of critics. It was a mind for observation and generalization, and he kept constantly up with the facts of the science, at the same time that no one surpassed him in the readiness with which he grasped and applied general principles. To sum up all in one word—*Dr. Drake was a philosopher.* He embraced in his capacious mind the whole ample range of medical science. He was eminently safe as a teacher, because he was cautious, candid, laborious and truthful. His style is uniformly simple, easy, and natural, sometimes loose and careless, but occasionally, when his subject favored it, ornate, poetical, and elevated. He had an active fancy, and his powers of description were of a high order, some of his pictures being models in their way. No one who has read it can ever forget his description of a "*doctor's shop*" as he was wont to contemplate it in the days of his pupilage, and as he still occasionally encountered it in his journeys through the country. The following description of the scenery of the Lakes is full of poetical beauty. He is describing what is seen from the ruins of Fort Holmes on the Island of Mackinac, and says:

"When the observer directs his eye upon the water more than upon the land, and the day is fair, with moderate wind, he finds the surface as variable in its tints as if clothed in a robe of changeable silk. Green and blue are the governing hues, but they flew into each other with such facility and frequency, that while still contemplating a particular spot, it seems, as if by magic, trans-

formed into another; but these mid-day beauties vanish before those of the setting sun, when the boundless horizon of lake and land seems girt round with a fiery zone of clouds, the brilliant drapery of the skies paints itself upon the surface of the waters. Brief as they are beautiful, these evening glories, like spirits of the air, quickly pass away; and the gray mantle of night warns the beholder to depart for the village, while he may yet make his way along a narrow and rocky path, beset with tufts of prickly juniper. Having refreshed himself for an hour, he may stroll out upon the beach to listen to the serenade of the waters. Wave after wave will break at his feet, over the white pebbles, and return as limpid as it came. Up the straits he will see the evening star dancing on the ruffled surface, and the loose sails of the lagging schooner flapping in the fitful breeze; while the milky-way—Death's Path of the red man, will dimly appear on the waters before him. Behind, in the street, a lively group of Canadian French of every shade of color between white and red, will gossip and shrug their shoulders; on one side, should the Indians, who still inhabit the shores of lake Michigan, be on a visit to the Island, he will hear the uproar of a lodge of drunken Chippeways, with the screams of women and children, and the cackling of frightened hens; on the other, will see the sober and listless Ottawa, sitting in silent vacancy of thought, on his upturned birch canoe; his wife within the tent spreading cypress bark and flag mats upon the ground, as lodgings for the night; while half a dozen children loll or play about the door, and as many half-starved dogs curl up among them. Surrounded by such scenes, the traveler begins to realize that he is a stranger; when suddenly a new phenomenon appears and fixes the conviction. Every object becomes more visible; and, raising his eyes, he beholds the heavens illuminated with an aurora borealis, where he reads in fantastic characters of strange and eccentric light, that he is, indeed, a sojourner in a strange land, and has wandered far from home and his friends in the sunny regions of the South."

Dr. Drake, as is too well known to the members of this society, died in Cincinnati, on the 6th of last November, having returned to the city of his choice, and to the school which he founded thirty-three years before. His great work was left incomplete, but in such a state that the second volume will soon be ready for the press. The presence of this eminent man at our last meeting will be remembered with interest by many who were present on that interesting occasion. We saw him there as he appeared a quarter of a century before—still vigorous, active, buoyant, full of enterprise and hope—a pleasing instance of faculties unimpaired at an advanced age.

JOHN E. COOKE, M. D.

It is natural to speak of Dr. Cooke in connection with Dr. Drake. They were for several years associated in the Medical Institute of Louisville. Both were at different times incumbents of the same chair in the Lexington and Louisville Medical schools. Dr. Cooke succeeded to the professorship of Theory and Practice of Medicine in Transylvania University when Dr. Drake resigned, in 1827; and when, in 1844, Dr. Cooke retired from that chair in the Medical Institute of Louisville, Dr. Drake was his successor.

Dr. John Esten Cooke was one of the most voluminous of the medical writers of Kentucky, having devoted several years of his life to systematic authorship. When he removed to Lexington, in 1827, he was engaged in the preparation of an elaborate work on Pathology and Practice,* one volume of which had already been published. The second volume was issued from the press at Lexington after he had entered upon his duties in the University. Before the appearance of this treatise, Dr. Cooke had made himself favorably known to the profession by one of the clearest and most philosophical essays on fever ever published.

The theory which he put forth in his treatise and repeats in several of his papers, is exceedingly simple, and has found in his pupils a host of advocates.

The first proposition he lays down is, that the remote causes of fever weaken the action of the heart, and that the principal of these are misamata and cold. The blood is blackened by the same causes, the quantity in the arteries is diminished, and an accumulation takes place in the vena cava or its great branches, in consequence of the weakened action of the heart. The effects of this accumulation are vertigo, headache, stupor and convulsions deranged secretion of the liver, kidneys, &c. The accumulation at length giving rise to convulsive agitation of the body, as in ague, the blood is urged vehemently on to the heart, which is excited to increased action by the increased supply of blood, and the hot

* A Treatise of Pathology and Therapeutics, 2 vols., pp. 800.

stage is developed. The difference between remittent and intermittent fevers arises out of difference in the excitability of the heart, which is so much greater in the former that the blood is not allowed to accumulate in such quantity as to produce an ague, as in the latter. All autumnal fevers are but modifications of one disease, and typhus, typhoid and yellow fevers, plagues, cholera, and dysentery are the same. Dyspepsia, liver complaints, and many of the diseases peculiar to females, depend upon congestion of the vena cava.

This is an outline of his pathology. His therapeutics are as simple—the great indication is to remove the congestion of the large internal veins; and this is to be effected mainly by cathartics. Occasionally venesection is beneficial; emetics are sometimes useful as evacuants; cold water is very effectual in reducing the action of the heart, and nauseants are also admissible with the same view; but the main reliance is upon evacuating the liver. Calomel stands incomparably first as a chollgogue, and is *the* remedy. It is not a matter of indifference what purgatives are given to aid its action—all are to be avoided that induce watery discharges. The passages must be consistent—bilious—and, after calomel, the articles most reliable for bringing them about are aloes, rhubarb, jalap, and scammony. The calomel is to be given in large doses and repeated until the evacuations become natural or the patient is well. In bad cases, Dr. Cooke says: "I have given with the best effect a drachm of calomel. It produces a copious, tenacious or viscid discharge, of a deep green or nearly black color, and affords the most marked relief." All the morbid phenomena depending upon congestion, the fever, whether intermittent, remittent, or typhus, give way when the accumulation of blood in the cava is removed by bilious purging. Hence, bark, tonics, and quinine are unnecessary. Salivation is to be avoided, if possible.

This, we believe, is a true account of Dr. Cooke's theory of Fever, and sufficiently full, we think, to be intelligible. In all his papers published after the appearance of his "Pathology and Therapeutics," he presents this view more or less in detail, and gives his treatment of disease as based upon it. The Transylvania Journal of Medicine contains many of his papers, and the

burden of most of them is this pathology and practice, in the soundness of which he had the most undoubting faith. His first article in that Journal contains the "Theory of Fever," of which we have presented an outline. His second essay* treats of cholera infantum—its pathology and treatment—which he refers to the same cause—that excites fever, and treats in a similar way. He combats with unanswerable arguments the notion that the complaint is caused by dentition, worms, summer fruits, or cold, or heat unaided. The cause is malaria, he shows by a multitude of instances. Calomel is the curative agent. "As soon as consistent passages are established, the copious thin discharges cease." "So striking is the quieting effect of a dose of calomel, that parents have often inquired if there was not opium in the powder. The medicine must be continued daily until the passages become entirely natural in color, consistence, and smell. The diet may be anything that the child desires and can retain."

In the same volume of this journal he has an essay on Autumnal Diseases, in which he goes elaborately into the remote cause, adducing an irrefragable mass of facts in proof of their miasmatic origin. It is one of the most logical efforts to be found on that subject. If Dr. Cooke has not established the position, no future writer need hope for success. He has cited facts from every medical writer who had touched on the subject, and presents them with singular clearness and force.

This essay is followed in the same journal, by one on "Winter Epidemics," which he traces to the same remote cause. Persons in a neighborhood where miasmata abound are still under their influence, many on the verge of an attack of fever at the access of cold weather. When winter sets in there is another remote cause of fever in operation, cold, which produces a continuance of the epidemic through the winter months.† Pneumonia and pleurisy are only additional symptoms, intervening in many cases, the real disease being bilious fever. For the ordinary cases, the remedies are the same precisely with those proper in the treatment of autumnal fever. Where there are pneumonic symptoms, it is necessary in addition to resort to bleeding and blistering.‡

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* Trans. Jour. Med., vol. 1, p. 59. Ibid. p. 190.

† Trans. Jour. Med. &c., vol. 1, p. 539. ‡ *Ibid* p. 550.

The second volume of this journal contains an essay by him on the "Remote cause of Typhus Fever, with some observations respecting its treatment." He opposes its contagiousness, but insists that it rises like the other fevers, from the decomposition of vegetable matter. "Typhus fever, therefore," he says, "is common autumnal fever or bilious remittent fever of England,—p. 85. Purgatives are the most useful remedies. He would not urge his patients to eat.

In this Dr. Cooke is opposed to the more matured experience of the present day, which condemns purgatives, and inculcates the administration of food at regular periods, whether the patient has appetite or not.

In the same volume we find from him an essay on Phlegmasia dolens; and one on the use of purgatives in the diseases of women. Phlegmasia dolens he treats by cathartics, as he does menorrhagia, uterine hemorrhage, &c., which he holds, are produced by that prevalent venous congestion which is, to his mind, the source of so much trouble.

The fourth volume of this journal contains an able and acute paper by him in which he examines Clutterbuck's and Broussai's theories of fever. He succeeds in demonstrating very clearly that one erred as much as the other in his location of a disease which has no topical origin. He was eminently successful in demolishing the structure which these gifted men had reared with so much ingenuity; but the theory which he would have substituted for theirs has gone into the same "limbo of vanity"—that *gurgite vasto* where so many theories of fever have been swallowed up and lost.

In the same volume he has a long, candid, and manly reply to some strictures on his Pathology and Therapeutics in the American Journal of the Medical Sciences.

The 5th volume contains two of his papers—the first, on the use of cold water in fever—an admirable paper, founded on experience, and inculcating a salutary practice; the second, on Spasmodic Cholera, of which at that time (1831) he had not yet seen anything.

The succeeding volume contains two papers from his pen. The subject of the first, is some of the diseases incident to a state of

pregnancy. He recommends the use of purgative medicines as mitigating the sufferings—the headache, nausea, vomiting, &c.—of pregnant women—a practice suggested by his theory of congestion, but which, there is reason to believe, will often be found to produce happy consequences.

His second paper consists of "Remarks on Cholera as it appeared in Lexington, in June, 1833."

This is a remarkable paper. It records the observations made by him during the progress of the pestilence in Lexington. He describes with singular clearness the topography of the town and the spread of the epidemic, and gives with faithful minuteness an account of his extraordinary practice. He labors hard to prove that the disease conforms to the laws that govern malarious diseases, and it must be admitted makes out a strong case. He shows that it broke out in just such weather as is most favorable to the generation of malignant fevers, and that it was most fatal in precisely those localities where such fevers are most apt to prevail. His argument is as strong as any ever produced in favor of the miasmatic origin of cholera.

But the most striking feature in this article is his treatment. He administered calomel to his patients laboring under cholera with a boldness that, perhaps, has never been equalled. In numerous cases he gave an ounce, and even a table spoonful, at a dose. One young man, William Douglass, 19 years of age, took a table spoonful every six hours for three days. Mercurial purging was never effected, and the patient died. But another patient recovered under similar treatment, and still lives to bear witness to the amount of calomel that a man may take without injury to his system. Mr. Britton took a table spoonful four times on one day, and three times the next, "and in the same time three other similar doses, having thrown up three." The doses were repeated immediately after the regular ones were rejected. Dr. Cooke adds, "he was somewhat salivated," but his final recovery was complete.

In recording this extraordinary practice we feel that we owe it to justice to testify, as we are able to do from personal observation, that Dr. Cooke was as successful in his practice as any of his brethren in Lexington.

Connected as Dr. Cooke was with a leading medical school, having a manner which at once convinced every hearer of the sincerity of his opinions and the truth of his statements, with great logical powers and a style of remarkable clearness, it is not surprising that through his writings and lectures his theory gained a wide currency, and his practice was extensively adopted. But a quarter of a century has passed away since his system was promulged, and we need not add that it has not escaped the fate that has attended every other system of medicine hitherto proposed. The theory is no longer defended by any of his pupils, and among them all, embracing many of the most eminent and skilful practitioners of the West, very few, perhaps, would venture upon his heroic practice. But let us be just to this honest and laborious cultivator of his profession. If his theory of disease has not stood the test of the severe scrutiny which awaits all medical speculation, his facts will remain, and his histories of cases form a record which will not lose its interest in all coming time. No one who reads these histories will doubt the perfect candor in which they are written, or fail to be struck with the lucid and simple style of the narrative. Honesty of purpose marks everything which has emanated from the pen of Dr. Cooke. It is visible in every page of his voluminous writings. He sought truth and truth only. He never contended for victory, but for principle. No man ever entertained fuller convictions of the truth of all that he inculcated. He was, happily, without a doubt of the soundness of his pathology and of the entire efficiency of his practice. His works are models of a clear, direct, simple style. It was his study to communicate his thoughts in the fewest and simplest words, and in a manner the most remote from circumlocution. He is as free from affectation as it is possible to be, and nearly as destitute of any semblance of art. One would look in vain through all his papers on whatever subject for a single specimen of an ambitious or fine style. He attempted no ornament, and seems never to have thought of rounding off a period. His sentences flow along smoothly, and pleasingly—as naturally as he would have spoken them around his own fire-side. It has been remarked, that few men can be trusted to write their own biographies. Dr. Cooke is one of the few who, without any detriment to his fame, might have

performed that delicate work. He would have written his life with all the honesty that he lived it—with perfect impartiality, keeping nothing back through a weak vanity, and exaggerating nothing.

Dr. Cooke died, at his farm, in Trimble county, the day on which this report was read, (Oct. 20th, 1852.)

DR. JAMES BLYTHE.

Each medical professor in Transylvania University feeling called upon to aid in the sustentation of the Journal of the school, James Blythe, D. D., Professor of Chemistry, wrote two papers for the initiatory numbers. Dr. Blythe was a Presbyterian clergyman of a strong and cultivated mind, and was one of the early presidents of Transylvania University. He was of a decided and even stern orthodox faith, having been reared in the same neighborhood, in N. C., with his colleague, Prof. Caldwell, to whom as an older and more advanced scholar, he was in the habit of relating, he was indebted when a lad for assistance in mastering many a hard lesson. He seems to have entertained serious suspicions of the orthodoxy of his early school-fellow, and to have felt himself called upon to keep a sharp watch over his prelections. Certain it is that there was a perpetual controversy between these teachers as long as Dr. Blythe remained in the school. The *odium theologicum* helped to point the arguments of the physiologist against the chemist; and the chemist, in turn, was harder upon the doctrines of the physiologist because of the suspicion that they tended to infidelity.

In his first paper (1828) Dr. Blythe treats of the "*Chemical changes that take place on the atmospheric air and upon the blood during respiration.*" He gives the leading facts on the subject as they were then received, in a calm, scientific, argumentative manner, which contrasts strikingly with the manner of his colleague on the same question. He says: "The whole of animal and vegetable developments are by a set of philosophers consigned to the government of a power denominated Vitality. But

we are not told by what law this power maintains its sway. Now this is the very thing we are in quest of, and we must still insist upon having those laws classed and explained. This vitality savours very much of the occult qualities of Aristotle." It is but too evident that he had here his physiological colleague in his eye, as he must have done when he penned the following passage in his second article, on respiration and animal heat:

"But suppose it should be said that none of these substances are found in the blood in a *free* or formal state. On this supposition I should like to know how we are to account for the two following experiments, viz: that the serum of blood changes the syrup of violets to a green, if blood does not contain *free* soda or some alkali; and if there be no free carbonic acid in the blood, how can we account for the fact, that newly drawn blood, under an exhausted receiver, gives it out in considerable quantities."*

We are not informed what answer the physiologist returned to these questions of his colleague, but we confess that they appear to us still unanswerable upon the hypothesis of the vitalists.

Dr. BLYTHE contends for Crawford's theory of animal heat, to produce which "he has no doubt, is one of the chief objects of respiration." Here again he was in antagonism to his ancient school-mate, of whom he speaks as "a gentleman of great philosophical pretensions," who claimed to have "caused Dr. COOPER† to abondon his ground" in support of the chemical theory of this process. Dr. BLYTHE thinks that Dr. COOPER was too ready to yield his ground, and we imagine the clear-sighted old philosopher came to think so himself before he died.

With these sensible papers, which havs not suffered at all by the subsequent progress of physiological science, Dr. BLYTHE's contributions to our medical literature seem to have ended, however much he continued to chafe his fiery colleague by assailing his peculiar notions as often as opportunity was afforded. He resigned his chair in the institution in 1831, and died, in 1841, in the 72d year of his age, having been for several years President of Hanover College, in Indiana.

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* Transylvania Journal, vol. i, p. 333.

† Late President of South Carolina College.

WILLIAM H. RICHARDSON, M. D.

The circumstance that drew Dr. Blythe for a short season away from more congenial studies to the public discussion of points in physiology brought out his colleague, Dr. Richardson, as an author. Dr. Richardson had been for several years a public teacher. As early as 1812 he gave lectures on midwifery to a few students in Lexington. In 1817 he was appointed professor of obstetrics and diseases of women and children, before he had taken the degree of M. D.; and on the organization of the medical faculty, in 1819, he was recalled to the same chair, which he continued to fill until he died. The second number of the Transylvania Journal contains a paper by him on "*Secale cornutum*," in the use of which his extended practice had afforded him much experience. He expresses the belief that the remedy, invaluable as it is, was too frequently resorted to, and that more confidence was reposed in it by accoucheurs "than comports with the legitimate objects of medical science."

In the second volume of this journal, p. 46, we find reported by him an interesting case of "Preternatural membrane in the vagina, requiring an operation during labor."

These brief papers give proof of the sound, judicious practitioner, but of one unaccustomed to making a public record of his experience. Dr. Richardson's use of language was not happy. He was not a scholar; but his oral delivery was fluent, easy, natural, earnest, and agreeable, and, without being a profound or scientific teacher, he was a popular medical instructor. He died, before his powers of mind or body had been impaired by age, of fever, on the 14th of September, 1845.

ART. III.—FOREIGN CORRESPONDENCE.

BY AUSTIN FLINT, M. D., PROFESSOR OF THE THEORY AND PRACTICE OF MEDICINE IN THE UNIVERSITY OF LOUISVILLE.

PARIS, June 26, 1854.

DEAR DOCTOR:—

One of the most interesting of the public institutions that I have visited, is the veterinary school at Alfort, situated about six miles from Paris. There are three establishments in France, under the direction of government, for veterinary instruction, and the treatment of the diseases of animals. That at Alfort is the largest. The two others are at Lyons and Toulouse. The institution at Alfort was founded in 1767, after a plan submitted by BOURGELOT.

Leaving Paris at 7½ A. M., accompanied by my wife, and a female friend, we reached the *école* at 8½ o'clock. The morning consultation was then going on. Several horses were standing in the court. They were examined, in succession, by the clinical professor surrounded by a large number of *élèves*, and several minor surgical operations were performed. The morning visit to the horses confined in the stalls had been made before our arrival. After the consultation, an adjunct clinical professor, M. REYNAL, a fine-looking, intelligent, and courteous gentleman, very politely invited us into the reception room, and, ascertaining our wishes, directed one of the *élèves* to show us the entire establishment.

We visited first the stalls, which were well ventilated, clean, and in all respects comfortable. Distinct apartments are provided for horses which it is desirable should be kept isolated.

We were then conducted to the anatomical cabinet. In the center of the room are very fine skeletons, united and complete, of the dromedary, the horse, cow, hog, etc. Arranged in glass cases are skeletons of frogs, chickens, ducks, monkeys, and a number of other animals; also, the disunited bones of the horse in profusion, all very neatly prepared and arranged. An adjoin-

ing room contains an artificial model of the horse, of the size of life, manufactured by AZOUX, the muscular organs capable of being detached; several natural preparations of the entire subject, the muscles dried and varnished, on one of the animals thus prepared, the natural skeleton of a female being mounted *à la mode cavalier;* wet preparations showing the different periods of gestation, and a large collection of deformed and diseased bones. I noticed in this room a fine display in spirits of the glands of *peyer* in the horse. The young *élève* who accompanied us stated that these glands, in the horse, are, normally, quite conspicuous. He also stated that they are found enlarged and ulcerated in certain cases in which the animals during life have appeared to be affected with a disease analogous to continued fever. This was to me new and highly interesting, suggesting the reflection that comparative pathology may perhaps prove as important in its bearings on our knowledge of human diseases, as the study of the structure of inferior animals has been of utility in the application of its results to the investigation of the organism of man.

In a third room was a large collection of concretions found in the stomach, intestines, and bladder of the horse and other animals. Also a series of excellent models, showing the appearance of the teeth at different ages; numerous injected vessels; a collection of minerals; different kinds of shoes for horses, etc., etc. In nicety of preparation and disposition, these several cabinets were not a whit inferior to the museums at the *école de médicine* of which I gave some account in my last letter.

We next visited the large botanical gardens connected with the institution. Here, among other classes of vegetable productions, were a great number of different species of grasses, each species being designated by its botanical title inscribed on porcelain.

From the gardens we walked through the park, beautifully ornamented with trees and shrubbery. Here were scattered some fifteen or twenty well dressed, fine-looking young men, most of them reading. Owing to my engagements on week-days, I had selected Sunday morning for my visit to Alfort, and our guide informed us that these young men were paying the penalty of having violated some of the rules of the institution, being obliged to remain at home, while their fellow-students had permission on h day to visit their friends.

The department appropriated to dogs was the next object of interest. In a small court, tied, were forty or fifty dogs, most of whom, the moment we entered, began to bark violently. Involuntarily we stepped back, until a scrutinizing glance showed that the animals, whose combined tones were frightfully ferocious, were perfectly secured. For the most part, they had been brought to the institution to be treated for *epizoa*. The animals seriously diseased were confined in kennels secured in front by iron rods. Here was a dog affected by *rabies*, the first that I had ever seen in which this disease undoubtedly existed. This dog was of the terrier species. It manifested violent irritation, barking, and making efforts to bite through the grating. There was no foaming at the mouth, but the teeth, on the contrary, were dry, and covered with sordes. The disease had been produced intentionally in this animal, by inoculating it with the saliva of a sheep affected with the disease. Our companion stated this curious fact, viz: the saliva from a rabid horse does not communinate the disease; but it is readily produced by inoculation with the saliva of a rabid sheep. At the access of the disease, convulsions are apt to occur, and death usually succeeds a comatose condition. I was surprised to learn that dogs live weeks, and even months, after the development of the affection. I was assured that there does not exist in dogs that inability to drink, or antipathy to fluids, which are presented in the human subject affected with hydrophobia.

An examination of the pharmaceutic department; the chemical laboratory and lecture-room; the apparatus for the illustration of physics; the general lecture-room, amphitheatre, dissecting room, and room for experimental operations, completed our tour of inspection. All these departments are as complete as possible. The arrangements for preparing and compounding medicines, and the chemical laboratory are really elegant, as well as complete. In the general lecture-room is a marble bust of BOURGELOT, the founder of the institution.

The number of *élèves* at present connected with the institution is two hundred and fifty. They all live within the walls of the institution. A portion enter as gratuitous pupils, on the recommendation of the minister of commerce and agriculture; others

pay a fee of about seventy-five dollars a year. Forty pupils are sent by the minister of war, and after leaving the institution, enter the cavalry service as veterinary surgeons.

They enter the school between the ages of seventeen and twenty-five, after an examination as to their ability to read and write correctly, and their acquaintance with certain elementary studies. They must also be practically conversant with the use of the forge. At the expiration of four years they are examined, and, if found competent, receive a diploma; if not competent, they are obliged to remain till the expiration of another year, or until the examination is satisfactory.

Horses and other animals requiring medical treatment are received at moderate charges, and gratuitously if the owners are poor. For a horse, fifty cents per day is the customary charge; and for a dog, twelve cents. A dog was admitted as I was leaving the institution, with, as the owner supposed, *ver solitaire.*

Horses unfitted by age, or other causes, for active service, are purchased for experimental observations, and to serve as subjects for the practice of surgical operations. Dissections are going on from November till April. Sheep and swine are also kept for experimental purposes.

The establishment embraces farriers' shops; and although it is not expected that the graduates of the school will be employed in shoeing horses, they must be qualified to know when it is properly done, to give directions as respects the operation, and to be able to do it themselves in cases of emergency.

The institution is placed under the immediate administration of a director, a steward, five professors, and three *chefs de service*, or adjunct professors. The departments of instruction belonging to the chairs severally are as follows; surgery and *clinique;* pathology; anatomy and physiology; chemistry and physics; agriculture and hygiene.

By means of this establishment and the two others already named, which, although less important, are conducted on the same plan, France provides her army, and the public, with a corps of scientific veterinary practitioners. With us, such establishments and such men are not to be found. Veterinary surgery, as a science and art, so far as my knowledge extends, does not exist in

America. A visit at Alfort, however, would, I think, impress you, as it did me, with the great importance of institutions for veterinary practice and education; and I hope the deficiency in this respect in our own country will not much longer exist.

Very truly yours,

AUSTIN FLINT.

PARIS, June 28, 1854.

DEAR DOCTOR:—

In addition to the regular courses of instruction at the *école de médicine*, which I have specified in a former letter, and the clinical lectures by TROUSSEAU, VELPEAU, NELATON, CAZENAVE, and others, at the hospitals, a great number of extra official or private courses are still going on in Paris. The courses in which I have personally been much interested are the lectures by Prof. BERNARD, on physiology, and the demonstrations in microscopy by Prof. ROBIN. Prof. BERNARD'S lectures are not extra-official or private, but they have no connection with the *école de médicine*. They are given at the *Sorbonne*, Prof. B. having recently been made a professor in the College of France.

The name of CLAUDE BERNARD is well known in the United States, not from his works, for I am not aware that any of the few publications that have as yet emanated from his pen have been translated, but mainly through letters by American physicians who have attended his lectures or followed his demonstrations in the laboratory. Among the letters referred to, those by Dr. DALTON, of New York, and Dr. DONALDSON, of Baltimore, are particularly worthy of mention.

Prof. BERNARD has for several years devoted his attention to *experimental* physiology, following in the footsteps of MAJENDIE, with whom he was long associated as an assistant. The *élève* has already outstripped his master. Probably at this moment no one is more conspicuous, or more justly distinguished in the particular domain of science with which his name has become identified; and if his life be spared, inasmuch as he has hardly, as yet, arrived at middle age, much may be expected hereafter from his continued zeal and industry. The discovery of a new function of the liver

is the most prominent among the fruits of his physiological labors. That this viscus forms sugar from the nutritious elements received by the portal vein, has been fully established by his numerous observations and experiments. He seems, moreover, to have proved that the production of sugar by the liver is truly a *formative* process, that is to say, it is formed in the organ by a union of the simple elements, not merely by a conversion into sugar of compound substances which are capable, out of the body, of being thus transformed. He has found that animals restricted exclusively to a diet containing only nitrogenized elements, present the evidences of sugar in the liver. The saccharine matter produced in the liver is destroyed by respiration. It is only found in a state of health in the blood-vessels between the liver and lungs. Prof. B. has also demonstrated experimentally certain curious relations between the presence of sugar in the blood, and the condition of the nervous system—but these, together with other highly interesting but less striking discoveries, have been described in the letters of Drs. DALTON and DONALDSON, and by others. I will only add, it is evident discoveries like that pertaining to the liver, are interesting and important, not alone in a point of view limited to physiological science. They are calculated, as are all striking discoveries in physiology, to exert not a little influence on pathology and therapeutics. Our notions with respect to *diabetes mellitus*, which have already experienced, during the last few years, several mutations in accordance with new light shed by the progress of physiology on the functions of the kidneys and of assimilation, must be materially modified in consequence of the grand discovery of BERNARD.

In the course of lectures now in progress, Prof. B., up to the time I am writing, has been engaged mostly in treating of the nervous system. His style of lecturing is unaffected, easy and lucid. Like all the French lecturers that I have heard, he is fluent. This advantage, at least, is secured by the system of *concouring*, since an aspirant who is notably deficient in ability to communicate his ideas, could hardly expect to triumph over his competitors. He uses the black-board frequently, and, whenever practicable, demonstrates the important points which he wishes to inculcate, by experiments on living animals. His course is numer-

ously attended, his lecture-room being generally quite full; and his auditory appears to consist of a large number who are not medical students, judged by their age and general appearance.

A work by Prof. B. on experimental physiology is announced to appear shortly. It will be sought for with avidity by members of the medical profession in all countries.

Prof. B. has just been honored by an election to the Academy of Sciences, to fill the vacancy occasioned by the death of the eminent surgeon, ROUX.

Dr. CHARLES ROBIN is professor *agregé* to the Faculty of medicine, his departments being general anatomy and microscopy. Within the last few years, several works relating to the subjects just mentioned, as well as numerous papers contributed to the Biological Society, have appeared from his pen. The elaborate treatise prepared by him in conjunction with Dr. Verdeil, entitled *Anatomical Chemistry*, is a production of a very high order, embodying the results of original researches, much being new, and in a practical point of view, important. Prof. R. appears, by common consent, to hold the first place in Paris as a microscopical observer. He has been a laborious worker, as his publications testify, and his industry is still unremitting. He is not content with what he has already accomplished, and it is certain that his scientific mission is as yet far from being fulfilled, provided his constitution, (which seems to be somewhat delicate,) does not give way in his unceasing application to his favorite pursuits.

A private course of instruction in normal and pathological anatomy by Prof. R. is about concluded; and its completion will be immediately followed by the commencement of another course. Three evenings a week are devoted to these lectures, which are given in his private laboratory. In addition to his descriptive course, he has always two or three classes for practical demonstration with the microscope. Each class is limited to four persons. They meet at his laboratory three times a week, in the afternoon, and examine with the microscope specimens of healthy and morbid products. With the latter Prof. R. is abundantly supplied by the various hospital physicians and surgeons who wish to avail themselves of his opinion as to the pathological character of diseased parts coming under their observation. M. ROBIN is en-

gaged in the composition of an elaborate and comprehensive treatise on general anatomy, normal and morbid, which will embrace a large number of plates illustrative of microscopical appearances engraved from delineations by himself of observations in his own laboratory. Through the kindness of M. R., I have examined some of the drawings destined for this work. They are very beautiful, and will supply a want which is now much felt by those who wish to place themselves *au courant* with microscopical anatomy and pathology, but who cannot conveniently avail themselves of the demonstrations of a practical microscopist. Moreover, in matters of doubt and discussion, and many such pertain to this subject at present, views emanating from so distinguished an observer will be very acceptable. The work will require two years for its execution, and when completed, I am gratified to say that it is the intention of the author to pass a year in the United States.

The medical topic which has excited most interest in Paris during the last two months, is the treatment of deviations in position of the uterus. The subject was brought before the Academy of medicine by M. VELLEIX, who communicated some cases in support of the treatment of certain deviations by intra-uterine pessaries, as practiced by Prof. SIMPSON, of Edinburgh. M. DEPAUL, in behalf of a commission appointed by the Academy, made a long and elaborate report, taking strong ground against the introduction of foreign bodies within the uterus, and submitting a collection of cases in which serious results had followed this plan of treatment. A protracted discussion in the Academy has followed, different members expressing different views, but on the whole, M. DEPAUL'S paper appears to be sustained. I have already sent medical journals containing M. DEPAUL'S report, and shall transmit subsequent numbers in which you will find the views of other distinguished speakers. A large portion of the columns of the various medical journals of Paris has been devoted to this topic for the last few weeks.

Having occasion to refer to medical journals, I am led to mention a melancholy event announced in the papers of this morning. Dr. FABRE, chief editor of one of the most popular and extensively circulated of the Parisian medical journals, day before yesterday,

while walking in the street, suddenly felt a sensation of faintness which led him to step into a *café*. He had hardly entered when he fell to the ground insensible, and expired in a short time. His death is attributed to sudden congestion of the brain. He was fifty-seven years of age. He was an able writer, and contributed largely to the French medical literature, in addition to his labors as a journalist.

The subject of hereditary syphilis is, at the present moment, under discussion at the meetings of the Academy of Medicine. A paper was recently communicated by M. CULLEVIER, one of the surgeons of the hospital Lourcine, in which he broached the doctrine that the syphilitic affections developed shortly after birth are invariably derived from the mother, the father bearing no part in their transmission. The correctness of this doctrine is contested by RICORD, and other members of the Academy. The discussions are too protracted for me to give you even a *résumé*, but I shall supply you with the medical journals in which they are reported, that you may examine them at your leisure.

Epidemic cholera has existed in Paris to some extent during the three months that I have resided here. For the past two weeks, the number of cases at the hospitals have increased. The disease has been for the most part confined to the hospitals. There does not appear to have been evidence of an epidemic influence except that nearly all the hospitals have had, much of the time, a few cases in the wards, and in some (*La Charité*, especially) a number of cases have been developed within the wards. Nothing is said on the subject in the papers, and I presume few of the citizens of Paris, aside from those who frequent hospitals, are aware that the disease exists in the city. The medical journals are careful to publish bulletins showing the facts with respect to the progress of the disease. The *Gazette des Hôpitaux* of the 24th inst., contains a list of the cases daily from the 15th inst., occurring at all the hospitals and hospices of Paris. The whole number for these seven days was 261. Of this number, 190 were received as cholera cases, and 71 were cases of persons attacked within the institutions. Of the 261 cases, 135 proved fatal, a proportion exceeding one half.

The same number of the journal just mentioned gives the whole

number of cases that have occurred since the month of November last. The number is 2,596. Of this number, 1,370 died, and 214, at the time the enumeration was made, were under treatment.

It is apparent from these statistics, that the disease is not in a special degree amenable to French therapeutics. The cases being distributed among thirty or more institutions, I am unable to say what is the plan of treatment generally pursued. I will give, however, the course pursued in one of the wards of the hospital *St. Louis*, in which I have been accustomed to make frequent visits, not directing, however, my attention to cholera cases, but to diseases of the skin, to which this hospital is specially devoted. My authority for the following statement of the treatment is the very intelligent *interne* of the ward referred to. As a special remedy, the physician of the ward places considerable reliance on the *Cannabis Indica*, the dose being, of the tincture, from 18 to 25 drops. But one dose only is usually given. Baths of mustard water are usually employed. Opium, in the form of the laudanum of Sydenham is prescribed in doses of 20 drops once during the twenty-four hours; and enemas containing 8 or 10 drops of laudanum morning and evening. Sinapisms to the epigastrium are relied upon to arrest vomiting. Ammonia is sometimes prescribed. My informant stated, as proof of the success of this practice, that five of twelve cases recovered.

Of French hospital practice in other affections than cholera, I may have something to say hereafter, if my leisure enables me to continue my correspondence.

With respect to hospitals, there is one fact (to which I think I have alluded in a former letter) at which I have been surprised. I refer to the custom of keeping full records of the cases from day to day. I had expected to find this course pursued at all the hospitals. This, however, is very far from being done. It is true that I have not yet visited all the hospitals, and I have directed my observations mainly to medical, as distinguished from the surgical wards; but so far as my present knowledge extends, I am able to mention but one instance in which I have witnessed a careful registration of clinical observations. This was at the hospital Lourcine, by M. GOSSELIN, one of the surgeons of that institution,

whom I have mentioned in a previous letter. I state this simply as a fact which I had not anticipated, and which, if I mistake not, will appear surprising to many of my fellow-practitioners in America. Perhaps the idea that the Parisian hospital physicians are in the habit of recording the symptoms of diseases by the bedside has prevailed with us, from our acquaintance with the labors of LOUIS, and the high estimation placed on them on our side of the Atlantic. I do not mean to imply by the latter remark that these labors are not appreciated by his own countrymen, but it is certain that his system and example are not generally followed in this metropolis. Certain facts are noted at all the hospitals, and I presume required by government, viz:—the age, residence, occupation, civil condition, disease, and result. In the wards of M. DUBOIS, at the *Hôpital des Cliniques de la Faculté de médicine*, I noticed that the *billets de salle* affixed to the beds of the lying-in patients contained data for statistics as to the previous number of pregnancies, the state of health during gestation, the duration of labor, the presentation, interval between the delivery of the child and the placenta, etc. But usually, the only daily records made, so far as appears, relate to the remedies prescribed and the diet.*

Bedside instruction is given here by several of the hospital physicians by means of what are called *clinical conferences.* A certain number of *élèves* inscribe with the teacher, and meet him at the hospital at his morning visit. He calls upon each in rotation to examine cases newly admitted, to put to the patient all the questions he chooses, percuss, auscultate, etc., and then to give the diagnosis and appropriate treatment. I have witnessed these clinical conferences at the hospital St. Antoine by M. ARAN, at La Charité by M. PIORRY, and at La Pitié by M. VALLEIX. With those at the hospital last named, I was especially pleased. After

* The following is a list of the several headings under which the daily directions are entered by the internes who accompany the physician, and afterward signed by the latter:

1. *Tisanes.* 2. *Medicaments internes.* 3. *Medicaments externes.* 4. *Bouillions.* 5. *Potages du riz ou vermicelle,* (a) *au gras,* (b) *au lait.* 6. *Soupes au pain,* (a) *graisse,* (b) *maigre.* 7. *Aliments solides,* (a) 1 *portion,* (b) 2 *portions,* (c) 3 *portions,* (d) 4 *portions.* 8. *Boissons alimentaires,* (a) *vin,* (b) *lait.*

the pupil had finished his examination, M. VALLEIX pointed out omissions in the interrogations or explorations, reviewed the facts, and, if necessary, corrected the conclusions at which the pupil had arrived. This is eminently practical instruction, and is certainly a most useful exercise for the student.

It is, however, attended with some inconveniences. The student is sometimes so timid and embarrassed that he requires to be continually pushed on; in some instances, too, his incompetency is so great, that he has to be corrected at every step, and, again, sometimes, his egotism and assurance lead him to ask a great multiplicity of needless questions, and to prolong the examination to a tedious extent. An improvement, as it seems to me, would be to require an examination to be made by himself at his leisure, and a written history, with the conclusions as respects diagnosis and treatment, to be presented, read, commented on and discussed at the morning visit. At a clinical conference by M. VALLEIX at which I was present on the day I am writing, two hours were consumed by the examination of two cases; and the examination of other patients was of necessity considerably abridged.

From the number and size of the hospitals in Paris, it would be inferred that the aggregate receptions in the course of a year must amount to many thousands. I have been informed that one-third of all the deaths which take place in Paris, occur at public institutions. In 1850, there were treated at the different hospitals under the control of "the general administration of public assistance," 88,949 patients. This is exclusive of the cases at the *hospices*, or alms-houses, and the *maisons de retraite.* If the latter were to be included, the number would be increased by 11,973.

The rate of mortality has steadily diminished during the last forty years. In 1805, the deaths were in the proportion of 1 to 5 at the general hospitals, and of 1 to 15 at the special hospitals. In 1850, 1 of 11 patients in the general hospitals died, and at the special hospitals, 1 of 17. The intermediate years show a progressive gradation in the proportion of recoveries, so that the above contrast is not to be explained by greater tendencies in diseases to a fatal issue during the two periods stated. It is a fair inference from these facts, that in hygienic conditions, or in the management of diseases, or in both combined, there has been continued

improvement. That there is room for farther improvement, which will reduce still more the rate of mortality, is undoubtedly true; but so far as concerns the general administration of the hospitals, the provisions for ventilation, cleanliness, nursing, etc., I must say I have been agreeably disappointed. I had not expected to find them so free from obvious unfavorable circumstances.

As you perceive, my dear sir, I am writing of different matters as they suggest themselves to my mind, and my letter will, therefore embrace a heterogeneous collection of topics. Reverting to cholera, a French gentleman deceased some time since, left a bequest of a considerable sum (I think ten thousand dollars) to be awarded by the Academy of Sciences to the discoverer of a cure for that disease. The hope of getting this reward has stimulated some Americans, not of the medical profession, to transmit certain recipes which they believe to possess unfailing specific virtues. Some letters enclosing these recipes were read at a meeting of the American Society, and as specimens of chirography and orthography, as well as on account of the singular character of the remedies and combinations communicated, they were sufficiently amusing. The evidence offered in behalf of the efficacy of the prescriptions was that they had proved successful in the persons of the applicants for the reward.

Passing from the cholera to the *Jardin des Plantes*, in a late letter in which I gave some account of the *Musée Orfila*, I mentioned that I had not then visited the cabinets of the former place. I have recently passed a portion of several days there, examining the collections in comparative anatomy, botany and geology, and zoology. To give any idea of the details embraced in these collections by a description within the limits of a letter, would be impossible. I will only say that to pass a week in the *Jardin des Plantes* seems to me, of itself, almost to repay for a voyage across the Atlantic. The assemblage of skeletons in the cabinet of comparative anatomy is immense. In this respect, the Orfila museum cannot be compared with it. In preparations of the soft parts, however, the Orfila museum is superior.

Courses of lectures on chemistry, zoology, botany, physics, comparative anatomy, mineralogy, etc., are in progress at the *jardin des plantes*, which, like the lectures at other public institutions, are free. At these lectures, ladies are admitted.

I desire to retract certain expressions in my first letter relative to the climate of Paris. For the first three weeks after my arrival, in April, the weather was delightful. During this time no rain fell, and so much danger to the crops was apprehended were drought to continue, that prayers for rain were ordered by the Archbishop of Paris. For the last two months, it is not much exaggeration to say that it has rained continuously. Certainly there have not been during this period, a half-dozen days of fine weather, and never have there been two days without rain in succession. Moreover, most of the time, fires and overcoats have been essential to comfort. Instead, therefore, of saying that even the elements of nature appear to have combined to render Paris an attractive city, it seems to me now nearer the truth to attribute its charms to the triumph of art over the natural disadvantages of climate. It is said here that such an unpleasant season was never before known, but it is well understood this is a very common method of speaking of unfavorable weather.

Very truly, yours,

AUSTIN FLINT.

Bibliographical Notices.

Art. IV.—*A Clinical Introduction to the Practice of Auscultation and other modes of Physical Diagnosis in Diseases of the Lungs and Heart.* By H. M. Hughes, M. D., Fellow of the Royal College of Physicians; Assistant Physician to Guys Hospital, etc. Second American from the second and revised English edition. Philadelphia: Blanchard & Lea. 1854. 12mo., pp. 304.

Dr. Hughes truly remarks, that it cannot be necessary at the present day to insist upon the advantages of auscultation and percussion to the practical physician, and to every legitimate practitioner of the healing art. The race of physicians who once dis-

credited these modes of diagnosis has well nigh passed away. There may be those among their successors, and we fear there are many, who still neglect them, but we doubt whether there are any who would have the hardihood to deny their utility. All that great improvement in the diagnosis of diseases of the chest made in the last forty years, is attributable to the introduction of these methods. But they require time, patience, assiduity, and perseverance in the student. Proficiency in their use is not to be acquired in a day, much less by intuition. A prolonged education is necessary to their skillful employment, but the facility that may be attained without such thorough training is useful, and the work of Dr. Hughes is an excellent manual for the student and practitioner who may not enjoy opportunities of applying them in hospital practice.

Dr. Hughes begins with giving all the directions necessary to the application of the physical means of diagnosis. Among others he lays down that—

"It is essential to the successful prosecution of auscultation and percussion that their value should be fully, but at the same time fairly, estimated. If the individual employing them be influenced merely by fashion, and labor under the misconception that they are in reality of little importance to the formation of a correct diagnosis, it is highly probable that, in his hands at least, they will prove so. If, on the other hand, the student conceive that by their use he will be enabled, in every case, to determine the exact condition of a diseased organ, he will be most assuredly liable to much disappointment. They should never be, and there is no valid reason why they ever should be, employed to the exclusion of the other modes of investigation. When the history, and the general or constitutional symptoms of disease, have been inquired into with the same minuteness, and interest as in other cases; then, but usually not till then, should percussion and auscultation be employed, or then, at any rate, and not till then, should any deductions be made from the indications afforded by the physical signs. To a neglect, or comparative disregard of the history, and ordinary symptoms of disease, and a too implicit confidence in their highly important art, it is probable that some of the errors of even accomplished auscultators are to be attributed. There is, however, I repeat, no valid reason why the auscultator should not use with equal diligence, and be equally skilled in the employment of all other modes of investigation, as the individuals who discard the stethoscope, and make no use of the ear in forming their diagnosis, and why he should not enjoy the advantage of his

art *in addition* to all those possessed by others. Let, then, auscultation and percussion be regarded as ancillary only to other, or rather as two among many, modes of investigating disease; as two stout and strong additional strings to our bow; albeit the information they afford is, in some cases, equal, if not superior, to that derived from all other modes combined."

Many truly valuable works have, within a few years, been prepared with a view to facilitating the study of auscultation and percussion in diseases of the chest, but we are not sure that one has been offered to the public containing more ample instruction in the same compass than this little volume, and our belief is that such readers as are in quest of a manual on the subject could not do better than to avail themselves of the guidance here tendered by Dr. Hughes.

Art. V.—*Description of a Skeleton of the Mastodon Giganteus of North America.* By. John C. Warren, M. D., late Professor of Anatomy in the University of Cambridge; President of the Boston Society of Natural History; Member of the American Philosophical Society, of the American Academy of Arts and Sciences, of the Philadelphia Academy of Natural Sciences, of the Academy of Naples, and the Medical Society Florence; Honorary member of the Medico-Chirurgical Society of London; Corresponding member of the Academy of Medicine at Paris, etc., etc. Boston: 1852. 4to., pp. 219

Remarks on some Fossil Impressions in the Sandstone rocks of Connecticut River. By John C. Warren, M. D., etc., etc. Boston: Ticknor & Fields. 1854. 8vo. pp. 54.

The author of these works has long stood prominent among the distinguished medical men of America. Dr. Warren has devoted a long life to the unwearied cultivation and advancement of his profession, in the midst of a laborious practice finding time to enrich its literature by a succession of works marked by sound judgment, learning, and originality. An able and successful surgeon and teacher, he has been a diligent cultivator of comparative

anatomy. More than thirty years ago he published a treatise on the *Nervous Systems of Men and Animals*, evincing accurate knowledge and deep research; and now, at an age when most men are content to seek repose, we find him following it up with a work on a kindred subject, which, if he had given nothing else to the world, would have placed him in the foremost rank of American naturalists.

Dr. Warren has been, for a number of years, the fortunate possessor of one of the most perfect skeletons of the Mastodon to be found in any cabinet, and to this circumstance we are, in part, indebted for this magnificent work. We rejoice that these interesting remains fell into the hands of one so competent, in every respect, to record the history of this monarch of our primeval forests. The volume is one of exceeding beauty and richness, and for the elegant form in which it appears we are wholly indebted to the munificence of its learned author.

No one, we should think, can be entirely indifferent to the history of this wonderful race of extinct beings, which at one period so abounded on our continent but long since has passed away, without leaving behind among the mammalian family one worthy representative. Their remains have been found scattered, in greater or less numbers, over a large portion of the United States and Canada. Herds of these huge creatures seem to have congregated, at times, about the saline springs of Kentucky, in the neighborhood of some of which vast collections of their bones have been discovered. The range of the Mastodon giganteus extended from the Connecticut river to the Willamet, in Oregon, where their skeletons have lately been found by an exploring expedition. It is a remarkable fact, recorded by Dr. Warren, that scarcely any of their remains have been known east of the Hudson river, and none east of the Connecticut, which cannot be explained satisfactorily by the severity of the climate, for Canada, still further north, presents a number of their relics. Some peculiarity in the soil, or productions of this section of the country, he thinks, may have been uncongenial to these animals; or possibly, he conjectures, New England may have been separated from the continent, in the time of the mastodon, by an arm of the sea too wide to have been readily crossed by the race.

It is a curious question what physical circumstances led to the extermination of this gigantic race. That they flourished at a period anterior to the human era there can be no doubt, since there is no tradition among any nation of men of their existence. They had become extinct before the present tribes of Indians took possession of America. By some it has been conjectured that they perished during the age in which the *drift* was spread over our continent, probably submerged under the great flood which brought down that deposit. But to this theory there is the grave objection, that their remains are found where there is no evidence of diluvial action. The drift ceases about the fortieth degree of north latitude, while bones of the mastodon have often been exhumed as far south as Alabama and Mississippi.

The fine specimen in the collection of Dr. Warren was found in the State of New York, near Newburgh, on the Hudson River, in the summer of 1845. Some laborers, engaged in removing the earth from the bottom of a pond to fertilize the fields of Mr. N. Brewster, struck their spade against some solid body, which on examination proved to be the head of the mastodon. "This, being uncovered, disclosed to the eyes of the spectators the full extent of the cranium, which was four feet in length. The lower jaw was distorted a little towards the left side. The bones of the spine, tail, pelvis, and ribs were successively found, for the most part in their natural relation to each other. The anterior extremities were extended under and in front of the head, as if the animal had stretched out its arms in a forward direction to extricate itself from a morass, into which it had sunk. The posterior extremities were extended forward under the body. The tusks lay with their convexities outwards, their anterior extremities opposed to each other nearly meeting." There, almost on the surface of the earth, scarcely covered by the soil and a few feet of water, they had remained undisturbed for unknown ages. The bones were well preserved, not black as are most of these remains, but of a brown color, like those of a recent human skeleton which had been a considerable time in use.

The bones of this creature when first discovered were supposed to pertain to the primitive elephant, but the mammillated character of the tooth soon convinced the great Cuvier, that they be-

longed to an animal of a different genus. In point of size the mastodon stood at the head of all the terrestrial mammalia. Dr. WARREN, in his faithful description of its osteology, says: "Language is insufficient to give an idea of the grandeur of the skeleton as a whole. Standing as it does in the midst of those of various large animals,—the horse, the cow, &c., and towering above them, its massive limbs make them sink into insignificence. Even the elephant, although nearly as tall, has a frame which might be called delicate when compared with that of the mastodon."

In the second of the works noticed at the head of this article, Dr. WARREN gives a description, which will be read with interest by geologists, of the fossil impressions in the sand-stone rocks of Connecticut river, preceded by an account of the *Epyornis*, or great Bird of Madagascar. The impressions to which Dr. WARREN refers are supposed to be, principally, the tracks of huge extinct birds, that in by-gone ages frequented the shores of that ancient sea. They constitute a study in which paleontologists have engaged with great zeal, under the names of Ichnology and Ornithicnites. To a geologist of our own country, Dr. HITCHCOCK, President of Amherst College, the scientific world is indebted for most of the facts upon which the newly created science of Ornithicnites rests. Besides the impressions of birds, the tracks of frogs, lizards, turtles, fishes, mollusca, crustacea, worms, and zoophytes have been perpetuated in the rocks of many countries, and the discovery of the former has served to carry back the creation of birds to an era much earlier than they were once believed to have existed.

But we must not run into details on this fascinating subject, which many of our readers, we fear, would deem rather foreign from medicine. In concluding this brief notice of these works, we cannot refrain from expressing our thanks to Dr. WARREN for the favor which their production has conferred upon American science.

Editorial Department.

SPIRIT RAPPINGS IN THE FRENCH ACADEMY OF SCIENCES.

Some of the readers of this Journal will remember, that an exposure of the pretensions of certain girls to communication with the spiritual world was made by Profs. Flint, Lee, and Coventry, in Buffalo, some years ago, and that these gentlemen, after a careful investigation of the sounds produced by the "Rappers," published an explanation of the phenomena, which was far from being generally received as satisfactory, at the time. For ourselves, we never for a moment hesitated to receive it as the true explanation of the mysterious sounds. In another part of this number will be found a case, reported to the French Academy, by M. Schiff, in which a female was able to create the rappings by the snapping of the tendon of the *peroneus longus* over the external malleolus. The subject was deemed by the Academy worthy of investigation, and a committee was appointed whose report was published in the *Gazette Hebdomadair* of July 7th. The identity of motion detected by M. Schiff in the young French woman, with that in the Fox girls, at Buffalo, was so striking that Prof. Flint, who was at the time in Paris, felt justified in calling the attention of the Academy to his observations, and he accordingly, through M. Rayer, made the following communication to that learned body. No one can now any longer doubt that the explanation offered by Prof. Flint and his colleagues of spirit-rapping is the true one. We subjoin his letter to M. Rayer.

Paris, June 28, 1854.

To M. A. Rayer, *Member of the Academy of Sciences, etc.*

Sir:—Having seen in the columns of the *Gazette des Hopitaux* notice of an observation and experiment relative to *spiritual rappers*, by Dr. Schiff, recently communicated by you to the Academy

of Sciences, I beg leave to ask of you the favor to transmit to the Academy an account of certain observations and experiments relative to this subject, with which I have been connected.

An exposure of this most remarkable imposition—an imposition which had for years baffled efforts at detection—was published in the United States, in the spring of 1851, by myself, in conjunction with two of my colleagues, viz., Dr. Charles B. Coventry and Dr. Charles A. Lee. The basis of the exposure was, that the sounds which it was pretended by the authors of the imposition (several females of a family named Fox) were made by the spirits of the dead, were due to the *voluntary muscles acting on the moveable parts of the body.* This principle embraced both movements of the tendons and articulations; and in the course of my investigations, an instance came under my observation precisely similar to that of the young girl examined by Dr. Schiff, and represented by Dr. S. himself before the members of the Academy.

The exposure referred to was published first in a public newspaper at Buffalo, at the time when the Fox girls were in that city holding levees for persons desirous of having intercourse with their deceased relatives. It was shortly afterward communicated to the medical profession in the pages of the Buffalo Medical Journal, of which I am one of the editors; and, subsequently, issued in a brochure. Neither of these printed papers are at hand, at this moment in Paris, but I solicit the favor of transmitting them to the Academy on my return to America. In the mean time, permit me to state the more important of the facts which they contain, which I am able to do perfectly from memory.

The city where I have resided for several years, Buffalo, State of New York, is in the vicinity of the place whence the imposition emanated. The latter is the city of Rochester, and hence the original spiritual rappers are commonly known, in America, as the Rochester rappers.

After visiting several parts of the country, the Fox girls at length honored Buffalo with a visit, and, influenced by curiosity, I was present at one of their exhibitions. A close examination of the countenances of the operators, and the method of conducting the exhibition, satisfied me that the sounds were dependent on the volition of the younger of the two sisters. In discussing the subject afterward, it seemed to me practicable to arrive at the basis of the exposure stated above by the process of reasoning so often useful in diagnosis, viz., reasoning by exclusion. Assuming that the sounds proceeded from volition, it is evident they must be produced either by the voluntary muscles acting on the moveable parts of the body, or by mechanical contrivances superadded to the body. That the latter were not employed might be inferred from the fact of the sound being heard under almost every variety of circumstance, and often repeated examinations of the clothing,

etc., leading to no discovery. Hence, by a logical necessity, the search for the explanation was directed toward the muscles, tendons and articulations, these being the only moveable organs under the control of the will.

By a coincidence very remarkable, and which I should attribute to a special interposition of Providence, had the exposure of the imposition sufficed to arrest its progress, a person who was present at the exhibition at the same time as myself, informed me that his wife was able to produce sounds precisely similar to those by means of which the Fox girls were deceiving the public. On visiting this female, I found his statement to be true; and the identity of the sounds as respects character, and intensity, was verified by my colleagues already named, and by several others. This lady permitted us to examine the mode by which she produced the sounds. She was able to effect at will, readily, and without pain, a lateral displacement of the knee-joint. The movement of the bones upon each other gave rise to a loud noise, which, on one occasion, was distinctly heard through a suite of apartments a distance of a hundred feet. She could produce a sound both in moving the bones from their natural position, and in effecting their return; or by causing the displacement or return to take place slowly, she was able to produce a single instead of a double rap. The movements in either direction could be effected silently and she could graduate the intensity of the sound according to the quickness and force of the movements.

In prosecuting further inquiries, I found another female who possessed an analogous faculty of producing sounds with the knee-joint; several instances of ability to produce them by different joints also came under observation, and, as already stated, I met with an instance in which the raps were made by displacement "*in tendon du muscle long péronier de la gaine dans laquelle il glisse en passant derrière la malliole externe.*" An account of the instance last mentioned, as well as of the other instances, is given in the publications made by me on this subject in 1851.

These cases were in themselves sufficient to render almost positive the correctness of the principle upon which the exposure by my colleagues and myself was based, viz., that the sounds were due to the voluntary muscles acting on some of the moveable parts of the body. But the infatuated assurance of the Rochester girls furnished an opportunity of rendering the demonstration of the correctness of this principle as complete as possible. Fearful of losing the notoriety and pecuniary gains which they derived from the imposition, they were induced to challenge us publicly, through the papers, to test the truth of our explanation. This challenge was accepted, and the following is a brief account of the interview which took place, by appointment, in the presence of several distinguished gentlemen. Prior to announcing what kind of experi-

ment we should adopt, the spiritual raps were very frequent; and, at our request, the *spirits* were asked if they would consent to hold communication with us, to which an affirmative answer was interpreted by the girls. Acting then, on a hint received from the lady who was able to produce identical sounds, she assuring us that she could not effect the necessary lateral movements unless her feet rested on the floor or against some firm pillar of support, we requested the *rappers* to take a position in the centre of the room on chairs, with the feet elevated and resting on the heels upon soft ottomans. A row of lights was placed on the floor before them in order to detect any sly mode that might be resorted to of violating the conditions of the experiment, and my colleagues and myself took a position for observation in front. Thus arranged the spirits were asked to fulfil their promise. At our instance they were importuned to adhere to their vow, but no sounds were produced. We waited patiently for nearly an hour, and offered to wait as much longer as was desired, and, indeed, did not consent to relinquish the experiment till the girls themselves declared that they had no expectations of the spirits rapping while they were in that position! Immediately on leaving the chairs and ottomans, and resuming their seats on the sofa, with their feet on the floor, the sounds began to be loud and frequent. On expressing freely our views of the success of the trial, the younger sister began to weep hysterically, and the *seance* was thus terminated.

I wish to add that since my attention was directed to this subject I have met with several persons who were able to produce loud sounds very analogous to the Rochester raps by suddenly and loudly retracting and extending upon the sole of the boot, or shoe, the toes; and it is very probable that the Fox girls, or some of their imitators, may adopt this method, which many, if not all persons may acquire by practice, it not requiring, as do the movements of knee and other joints, a peculiarity of conformation. This method, however involves the same principle, viz., the action of the will on muscles and moveable parts. Among the numerous rappers that have sprang up in different portions of the United States, a great variety of means for producing sounds exterior to the body are resorted to, which, however, are generally easily detected. But sounds produced within the ornanism, by the joints and tendons, were well calculated to elude the discovery of their source, inasmuch as this subject appears hitherto not to have received any scientific examination.

The extent to which spiritual rapping belief has prevailed in the United States offers a sad illustration of the credulity of mankind. Among the ranks of believers are not only the ignorant and persons deficient in mental endowments, but not a few of the educated and persons of high intelligence. A late distinguished incumbent of the bench, and also an ex-governor of one of the

territories of the United States are its avowed and ardent advocates. Many persons have adopted opinions and regulated their conduct in most important matters by information which they supposed in this way they received from spirits; and eminent clergymen have sought through this channel for enlightenment as respects the theological tenets which they should receive and inculcate. The results of belief in the imposition have already been very grave. A large number of cases of insanity and suicides occurring in different portions of the country have been fairly attributable to it. It is, therefore, certainly a matter of sufficient importance to claim the attention of scientific bodies. This view of the subject, as well as considerations of a personal nature, have led me to desire, to avail myself of the occasion which has now offered itself to present the facts contained in this brief memoir to the Academy of Sciences.

With great respect, yours,

AUSTIN FLINT, M. D.

MEDICAL SOCIETY OF KENTUCKY.

We hope our readers in Kentucky will not forget, that the State Medical Society is to meet at Covington, on Wednesday, the 25th of October. The last annual meeting was not well attended, and it becomes the more necessary for those who feel an interest in the success and perpetuity of the Society to exert themselves to secure a good attendance at the approaching meeting. No one can doubt the utility of such associations, and those who have been present at former meetings of the Society will testify to the interest that has characterized its proceedings. We owe it to our profession, and to the character of the State, to sustain our Society, which opened under auspices so encouraging. The preparation made by committees authorizes the belief that the meeting in Covington will be one of the most interesting yet held. We hope the attendance will be large.

Record of Medical Science.

ELKOPLASTY; or, Old Ulcers treated by Anaplasty. (*Read before the Buffalo City Medical Association, June* 27, 1854.) By FRANK H. HAMILTON, A. M., M. D. Professor of Surgery in the University of Buffalo, and Surgeon to the Buffalo Hospital of the Sisters of Charity.—Some writer has said, "old ulcers in 1830 will be old ulcers in 1860," which, not to be understood always in a literal sense, was intended only to express, in a brief and pertinent form, the proverbial obstinancy of this class of sores.

In most cases, the integument has been broken and destroyed by ulceration, and then, usually, bad health, or, perhaps, enlarged veins. have helped to perpetuate the lesion. In other cases, however, the ulcers are directly in consequence of severe lacerating injuries, which have at once torn away the skin beyond the power of nature to repair; and that although the health of the body and of the limb may be perfect. In such cases, the refusal of the ulcers to heal is entirely owing to the extensive loss of integument.

Actual loss of skin is repaired by one or both of two processes. By the development of new, from or upon the free margin of the old skin, or by the contraction of the granulations and of the cicatrix, in consequence of which, the adjacent skin is drawn towards the chasm, and made, as it were, to slide over and cover it in.

This rule admits of but few exceptions. Occasionally, after a very long delay, the granulations acquire the power of forming new skin at various and isolated points of the sore. This process may now and then be observed in the healing of extensive burns, or, perhaps, in the closing up of an ulcer whose surface is excluded from the air. New skin may even find a matrix in the periosteum, as I have witnessed, and maintained several years since. (*Buffalo Medical Journal*, vol. vii. p. 205.) But the conditions are very rare under which these exceptions can occur. The rule remains as we have stated, and if ulcers are not closed by either the projection of new skin from the margins of the old, or by the contraction of the granulations and cicatrix, then, usually, they must remain open. To the action of both of these processes there is, however, a limit. The formative power of the old skin does not extend beyond a few lines. The new vessels, becoming more and more attenuated as they stretch inward from the peri-

phery, lose, at length, the power of generating epithelial cells, or, if formed, they are too imperfectly organized to sustain an existence, and they crumble away from the slightest provocation. Slowly, but perceptibly, the opaque diaphragm proceeds to shut in the granulations, and for a long time encourages a hope that a cure is to be accomplished. But just when the work is almost consummated, a rapid disintegration sweeps away in a few hours the patient labor of many months. Again and again the reluctant labor is renewed, and as often suddenly, and without provocation, is it arrested and broken up. At the same time the granulations have ceased to condense, and the cicatrix to contract, either because these actions have attained their natural limits, or because the adjacent skin has reached its utmost tension, and affords effectual resistance to further stretching. Here the process of closure forever ends, and the "old ulcers of 1830 will be old ulcers in 1860."

Nature has done its utmost, and hitherto art has failed to complete the work.

I beg to suggest a procedure, which, hereafter, in some unfortunate cases of this class, may deserve a trial.

I propose to close the ulcer by an operation of anaplasty. In short, to imitate one of the processes of nature, by sliding in old skin to repair a waste, where the process of forming new skin has ceased, and been finally given up.

If we seek to obtain this supply from the neighborhood of the ulcer, around which the skin has already reached its utmost tension, we shall only substitute one ulcer for another. We must, therefore, generally look to the opposite limb, or to the limb of some other person, for the material with which the transplantation or engrafting is to be made.

The mode of accomplishing this, will not differ materially from that which has been generally adopted in anaplasty from remote parts, except that the ulcerated surface ought to be excised freely before the new skin is laid upon it.

By this means, I hope, gentlemen, not only to supply an amount of skin equal to the size of the piece transferred, but to furnish, also, a nucleus from which additional skin shall be formed. I hope to establish a new centre of life—an oasis—from whose outer verge a true and healthy vegetation shall advance in every direction over the exhausted soil.

It is not improbable, also, that the graft will itself expand, or be drawn centrifugally by the contraction of the surrounding granulations and cicatrix, conversely, as the skin about the ulcer had before been stretched and drawn centripetally, by a similar action of the granulations and cicatrix situated within its free margin, so that, after a time, it will cover more space, independent of any actual growth, than it did originally. The opposite of this hap-

pens usually in anaplasty, and would occur here, did the flap equal or exceed in size the wants of the parts to be supplied. The flap would contract, thicken, and project itself above the surface. But in old ulcers, it will generally be found impossible to furnish a direct supply of integument equal to the loss. A deficiency must probably still exist, and sufficient, it is believed, to determine in the transplanted skin a necessity of expansion.

The value and practicability of these views are, I trust, in a measure established by the results, in the case which I shall now take the liberty of bringing before you.

You will excuse me, however, if I detain you a moment longer to explain to you that, so long as eight years since, I proposed the same operation, and had anticipated most of the results which I have now actually obtained.

In the report of my surgical clinic, for 1846, at Geneva Medical College, published in the *Buffalo Medical Journal*, vol. ii, p. 508, occurs the following passage:—

"*Indolent Ulcer.*—M—— of Geneva. This lad, now about fifteen years old, had the right leg and part of the thigh terribly lacerated, and almost deprived of its integument, by a threshing-machine, eight years ago. The wound has never closed entirely, but an indolent ulcer of great extent exists, surrounded by a broad margin of hard integument, from which sometimes new skin will form, and then it will rapidly crumble away, and the ulceration will extend, perhaps, beyond its original bounds. Thus it has continued to partially close and again open, during all this time; meanwhile, the health and strength of the lad have remained excellent, but the leg has become bent at the knee, and he walks with a halt. Two years ago Dr. Hamilton took a cast of the ulcer which is now seen to correspond almost precisely with its present extent.

"Dr. Hamilton and others having tried almost every plan of treatment which would offer a prospect of success, and having so completely failed, as Dr. Hamilton believes, because the indurated margin, near two inches in breadth—all around the sore, is incapable of projecting from itself sound skin, the Dr. has proposed to the boy a plastic operation, with the view of planting upon the centre of the ulcer a piece of new and perfectly healthy skin. He proposes to take this from the calf of the other leg (having secured the two together), not intending to cover the whole sore, but perhaps two or three square inches, which he believes will be enough to secure the closure of the whole wound in a short time."

Two years before the date of this clinic, when I took the cast alluded to in the above report, I had made the same proposition to the lad, and when he declined submitting to it, I appealed to his father, who was a worthless inebriate, to allow me to secure one of his legs to his son's that I might make the transplantation from

him. In no other way, I assured him, could he so much benefit his family.

I need scarcely say that permission was never obtained, and that I have never found an opportunity of determining the practicability of my suggestions until during the last year, and in the person of the man who is now before you.

The following is the report of the case, copied, in part, from the Hospital Records:

Horace Driscoll, aged 30 years, an Irish laborer, had the skin and flesh extensively torn from the right leg by a dirt car, on the 3d of November, 1852. He has been in the hospital most of the time since then until now. The wound has nearly healed several times, but never entirely; after exercise the whole would give way and the ulcer again extend itself completely around the leg.

Jan. 21, 1854.—I made the following operation:

The patient was laid upon his belly, upon the operating table before the class. A flap of skin measuring seven inches by four was then raised carefully from the calf of the opposite leg, extending in depth through the cutaneous and cellulo-adipose textures, until the fascia was in sight. Its remaining attachment to the body was by a broad and thick base. The hæmorrhage was slight; no vessels were tied. Lint, spread on both surfaces with simple cerate, was laid between the flap and the surface from which it had been detached, other pledgets of lint similarly covered were placed on the outer surface, while over all and around the entire limb was wrapped a large mass of cotton batting, secured in place by a lightly turned roller.

He was then laid in bed and perfect quietude enjoined.

Jan. 22.—During the night the wound has bled until the patient looks pale from the loss. The bleeding has now ceased.

Feb. 4.—Two weeks since the flap was raised. The patient has had to be sustained with beer, his appetite having failed very much since the operation. The flap has been dressed in the same manner as at first, nearly every day. It looks healthy. No part of it has sloughed.

To-day the operation was recommenced before the class, by dissecting out the granulations and part of the cicatrix from the diseased leg, and thus forming a deep bed of the size and shape of the flap as it now appeared, both contracted and thickened. The flap was then made raw again on its margins, and its lower surface was shaved off, with the double purpose of removing the granulations, and of diminishing its excessive thickness. When the bleeding had ceased, the left leg was carried across the right, so that the tendon Achilles and heel of the left leg rested upon the instep and ankle of the right—a thick cotton pad being interposed to prevent painful pressure. The flap was now brought snugly into its new bed, on the right leg, and well secured with inter-

rupted sutures, a moderate compress, and roller. The two limbs were further secured immovably to each other by bands, and protected at various points by well-made compresses, and the wounds carefully covered with lint spread with cerate.

Feb. 5.—The wound has bled again, as after the first operation, although ice was applied diligently from the moment the dressings were completed. Much pressure was regarded as inadmissible. Bleeding ceased when he became faint, about three hours after the operation.

Feb. 18.—Two weeks since the last operation, and four weeks from the first. Patient has required to be sustained constantly with beer and nourishing diet. His appetite still remains bad. Bowels have not been moved in two weeks. He has not suffered much pain, only fatigue. To-day the base was separated from the left leg, the flap having united through most of its edges and under surface, to the opposite leg. No bleeding of consequence followed. The parts were thoroughly washed and dressed with ung. basil. and a snug roller applied. Ordered sulph. mag. ʒj.

Feb. 19.—No movement of bowels. Repeat sulph. mag.

Feb. 20.—One corner of the extreme end of the flap is beginning to slough.

Feb. 21.—Bowels have moved. Sloughing of flap continues. Ordered yeast poultice.

Feb. 25.—Line of demarcation formed, insulating about one inch and a half of the flap, at the corner where the sloughing commenced.

Beyond this the sloughing never extended. The surfaces continued to close, and about one hundred days after the flap was laid down the healing was finally consummated, and now after a lapse of nearly three months, during which he has been acting as a subordinate dresser at the hospital, the ulcer has not re-opened or shown any tendency that way.

The wound made by the removal of skin from the left leg was completely healed over in about the same length of time as the ulcer on the right, and the whole left limb is now as sound and as perfect as before the operation.

Driscoll is, however, at present, by no means a well man. His health has suffered considerably from his long illness, and from his prolonged confinement in bed, which dates from the time of the accident, through most of the period, up to the time of the closing of the wounds since the operation. The cicatrix around the new skin is tender, and especially at one point where several pieces of bone exfoliated soon after the accident, and precisely over which, unfortunately, the sloughing of the flap took place. The ankle is also somewhat stiffened by the contraction of the skin, and of the gastrocnemii and tendon Achilles, which latter were seriously involved in the original injury. These, however,

are conditions which the operation did not propose to remedy, at least only in a small degree, or they are temporary accidents, and will certainly yield to time and careful use. If they were to continue, however, it will not be denied that, in the permanent sealing up of a sore, which, but for this operation, must probably have remained open during life, he is amply repaid for all that he has suffered at my hands. I venture to predict that, within one year from this time, he will be able to labor nearly or quite as well as before the accident.

On the 12th of March, five weeks after the flap had been transplanted, it had united by adhesion to the adjacent skin, through about one half of its circumference. The other half was surrounded by a border of granulations and of new skin, varying in breadth from one to ten or fifteen lines; but only at a few points was the bridge of new skin complete. It was especially noticed that nearly all, probably nine-tenths, of this new skin had sprung from the margins of the flap, and only the remaining fraction from the adjacent cicatrix; demonstrating that after transplantation and complete separation from the parent limb, its vitality was unimpaired, and that its re-productive power, if I may so speak, was vastly superior to the re-productive power of the old cicatrix.

You may notice to-day also, that since the cicatrization was completed, the cicatrix formed by growth from the flap, has contracted; and, that, in consequence of this contraction, the flap has become expanded, or been stretched outward, and its surface has become flattened and firm, whereas, it was, at first, and for a long time, elevated above the surrounding skin, and flabby.

Summary:—1st.—Ulcers, accompanied with extensive loss of integument, do generally refuse to heal, whatever may be the health of the body or of the limb.

2d.—Anaplasty will sometimes succeed in accomplishing a permanent cure, and especially where the health of the body and of the limb are perfect, and where, by inference, the refusal to heal is alone attributable to the extent of the tegumentary loss.

3d.—The graft must be brought from a part quite remote; generally from an opposite limb, or from another person.

4th.—If smaller than the chasm which it is intended to fill, the graft will grow, or project from itself new skin to supply the deficiency.

5th.—It is not improbable that the graft will expand during the process of cicatrization at its margins, but especially for a time after the cicatrization is consummated.

6th.—In consequence of one or of both of these two latter circumstances, it will not be necessary to make the graft so large as the deficiency it is intended to supply.—*N. Y. Jour. of Med.*

Lectures on Fevers. By William Stokes, M. D., Regius Professor of Physic in the University of Dublin. *Lecture* IV.—It was my intention to have commenced to day the examination of the more important secondary diseases of fever; but we must postpone this part of the subject, and take the opportunity of studying the general symptoms of fever as they are presented, in their most aggravated and appalling form, in the case of the young man Hardcastle. This patient is a native of England, and has been but a short time in Dublin, and his case is full of instruction.

I have mentioned, that this patient is not a native of Ireland, because it is probable, that this circumstance has acted in modifying the symptoms of his disease. Had this patient been attacked with fever in his native place, he probably would have shown the symptoms of what is called typhoid fever, and the case would have been a comparatively mild one. But, at all events, this much we may believe, that different countries present endemic, sporadic, and even epidemic fevers, with characters in some degree peculiar to themselves; and we are not yet able to explain, why fever in one country differs so much from fever in another—why maculated typhus should be so common in Ireland, and so rare in England; nor why the fever in Paris should so constantly present a particular local lesion. Many causes, doubtless, act; and I suppose among others, such as climate, diet, soil, and so on, that the race and temperament are to be reckoned. And I have suggested that, if the various families of mankind have a physiological stamp, they, probably, have also their pathological peculiarities; however this may be, it becomes a curious subject of inquiry, to determine what are the modifications of the symptoms of the fever in Ireland or in any other country, when the patient is not a native of the country, and especially if he has not been long resident in it. The most extraordinary case I ever witnessed was that of a gentleman, a native of France, who, after a long exposure to contagion, contracted maculated typhus here. He was a man of a sanguineo-nervous temperament, and exhibited, during his illness, a succession of symptoms widely different from those which we commonly see in the spotted typhus of this country. He was profusely maculated, and the peculiarity of the case consisted in the irregular manifestations of various local symptoms, and the inconstancy of the general condition, especially with reference to excitement or collapse. This gentleman happily recovered, after passing through a storm of disease, such as I and his other attendants, Dr. O'Farrel and Dr. Graves, had never seen before.

Hardcastle is a young man who has been well educated, and he is evidently above the rank of the ordinary hospital patient. His symptoms have been, from an early period of the disease, alarming in the highest degree. You will seldom see a worse case of adynamic fever. We may safely say, that for the last fort-

night, both by day and by night, there has been an uninterrupted struggle between medical art and the fell disease which is upon him. During that time he has been lying more like a decomposing corpse than a living man; but he has been kept from dying, by the bold and constant exhibition of stimulants, by tonics, and by food.

Some of you may wonder why it is that we thus go on with the increasing exhibition of stimulants—the untiring efforts to support life—although for the last ten days, at least, we have been hourly expecting his death? And the answer is, that we believe his disease to be one which may subside, sooner or later, under the law of periodic action. His recovery will then take place,—not in consequence of the action of any specific which has the power of curing fever, but from causes the nature of which is hidden from us. We are like the defenders of a post attacked by a powerful enemy, but yet expecting succor, and we seek to hold the place until that succor arrives.

There are two points in this case worthy of notice. One is, that the nervous system has been so little engaged. He has had occasionally a slight delirium, and latterly some agitation and slight subsultus tendinum; but, though he generally appears sunk in stupor, it is not true coma, but rather the stupor of exhaustion, for, when you rouse him, his intelligence appears to be good. Is this escape of the brain to be looked on as favorable, or the contrary? According to an old and well-founded opinion, all anomalous circumstances in fever are to be feared. You know the aphorism, "*Pulsus vultus et urina, bona, et æger moritur.*" This contains an important truth; but, in typhus, the want of nervous symptoms is in general favorable; for of the symptoms referrable to one of the three cavities, doubtless those indicating nervous lesion are the most formidable.

The second point to which I wish to direct your attention is, that, although this patient presents the symptoms of typhus so decidedly, he cannot be said to have any maculæ or petechiæ. In fact, his skin has been unaffected; there has been none of the so called exanthematous eruption of typhus; and, even at this advanced period, the existence of petechiæ is so doubtful, that I would not say that he has them. Yet, if ever a man had the *typhus gravior*, this man has it. His surface exhales the peculiar odor of typhus, strongly marked; he is prostrated to the last degree; he has been kept alive by the most powerful stimulation; he has a feeble heart, and well-marked secondary affections of the intestinal and bronchial surfaces; his mouth is full of sordes; his tongue black, dry, and cracked; his breath fetid. The stethoscope indicates a general and severe bronchial affection, and there is a wasting diarrhœa.

I am not going to take up your time by a discussion as to the

distinctive characters of typhus and typhoid fever, about which so much has been written of late. But it would be, I think, difficult even for the advocates of the distinction, to declare in which class this case should be placed. In this instance, as in most others, the settlement of the question is of little value; for, whether the disease be typhus or typhoid fever, the treatment should be no other than that which we have so far carried out. I may say this much, however, that the presence or the absence of an early eruption having some similarity to an exanthem; or, again, the presence or absence of petechiæ, appears to be insufficient to justify our drawing a strong line of distinction between these diseases. As to the doctrine, that the early eruption in typhus is a true exanthem, and that its absence in any given case points out that the disease is not typhus, but typhoid fever, I would advise you to receive it with more than caution. Again, it is stated, that the non-maculated fevers are often protracted and dangerous; so they are, but not so often as they are short and easily managed. And I have not seen that relief of organs in typhus, which some describe as the rule, when the eruption appears. How, then, are we to look on the eruption? Certainly not as a distinctive mark between two different diseases. Gentlemen, we know that typhus fever will run its course, and destroy life, without the production of any hitherto ascertained anatomical change. We know further, that in many cases, local alterations are formed; but that there is the greatest inconstancy in different epidemics, and in different patients during the same epidemic, in the seat, amount, in the time of appearance, and in the results of these local changes. The cutaneous affection is plainly one of this group of secondary affections; and its absence or presence can no more be said to distinguish the disease, than the absence or presence of any other of the secondary affections. I think we may admit, that an early and florid eruption is often met with in cases in other respects well marked; and again, that such cases generally require and bear stimulation.

To return to the case of Hardcastle. We observe that there has been as yet no tendency either to the critical or periodical retrocession of the secondary diseases, nor to the critical or periodical termination of the general affection. The fourteenth day passed by without any crisis or without any subsidence of the disease; the eighteenth day passed by; the twenty-first has passed by; and he is now in the twenty-fourth day of this terrible disease, kept alive by the use of wine, administered every hour, and also by the occasional free use of hot brandy punch. He presents an appearance to-day to which I wish specially to direct your attention. You may remember that Saturday last was the twenty-first day, and it was then reported to me that the patient was getting a bed-sore. Upon examining him, we found, in the ordinary sit-

uation of bed-sores, a blush of redness and a slight degree of œdema; and beyond that there was nothing remarkable. There was this point, however, which I observed at the time although it was not until to-day that I thought it of importance, namely, that there was already a solution of continuity of the skin. Now, this is remarkable; and if such a case should occur to me again it would awaken my attention. In the ordinary cases of bed-sores, we seldom see a solution of continuity of the skin in an early period; we see lividity, blackness if you will; but a solution of continuity of the skin before the ordinary appearances of mortification have occurred, is extremely rare.

In this patient, on Saturday, just at the very centre of the livid patch on the skin, there was a slight solution of continuity. During the evening, the nurse observed that several dark-colored vesicles or pustules were making their appearance on various parts of his body. Mr. Parr saw these on Sunday, and he found that between the shoulders there was an eruption of livid tumors, which appeared to be something between vesicles and pustules; and to-day (Tuesday) such of you as saw the patient will not soon forget the extraordinary appearance of his back. I have been attending on fever in this Hospital since 1827, and I never saw anything of this kind before. Now, it is a very remarkable circumstance, that this eruption of gangrenous vesicles or pustules should have occurred upon the twenty-first day; and this case, as far as it goes, appears to strengthen an opinion which I have long held, that we were in error in attributing what are termed bed-sores in fever, simply to mechanical causes. The general idea is, you know, that they are simply the result of pressure long continued on a particular part of the body, from the neglect of turning the patient in bed, of pressure combined with the effects of position. Now, there can be no doubt that these are predisposing causes, but whether they are the sole and entire causes is another matter; and I am, to a great degree, convinced, that the bed-sore in fever is often one of the group of secondary affections analogous to the ulceration, or to the tumefaction and ulceration, of the glands of the intestine—analogous to the bronchial affection which occurs in the middle stages of the disease,—analogous to the other secondary organic diseases of fever. And if analogous to them, it should be more or less observed to be under the law of periodicity; it should appear at a certain time, and it should disappear at a certain time, or, at least, exhibit a tendency to do so. We have observed this very curious fact. There are cases in which the system seems to be in a state that bed sores will form from the slightest possible causes, that is to say, that whenever there is any irritation or any pressure, no matter how slight, a bed-sore will form in any part of the body. Some years ago we had a patient in this hospital, who, after the twelfth day, showed this tendency

to bed sores. She had bed sores on the nates, and one on each shoulder. Then the tendency to the multiplication of these bed-sores increased, and every morning two or three new local diseases of this kind were discovered. Wherever there was the slightest possible pressure, we found a gangrenous spot; in the fold of the arm, in the fold of the pectoral muscle, where the mamma leaned upon the arm, where the head leaned upon the hand in sleep, the mark of a black hand was stamped upon the face,—where one leg lay on the other,—in every possible position in which anything like pressure was made, or irritation excited, there were bed-sores; and this went on repeating itself, day after day, up to a certain day, until there were about thirty sloughs in different parts of the body; and from that day,—I might say from an hour of that day,—no more bed-sores formed, and the patient then had to go through the process of throwing off all those sloughs the granulation of the cavities from below, and their cicatrization. She was kept lying on her face for upwards of a month, and finally recovered.

Now, what I want to draw your attention to in the case of Hardcastle is, that this extraordinary eruption of gangrenous patches on his back is not of the nature of bed-sores in the ordinary sense of the word. These patches are not produced by pressure. We find them in great abundance in the hollow of the back, where pressure is relieved by the pelvis and by the shoulders. We find them in abundance in the interscapular region; but if we wanted additional proof, it is this,—that we find them on the anterior portion of the thorax. They came out as vesicles. These vesicles became hard at the top, then black, and soon the mass dropped out; and this patient's back in now as if you took a sharp gouge and punched out circular portions of the flesh in a vast number of spots. Now, it can be hardly doubted, that this singular appearance is simply an example of a secondary disease affecting the surface; and it is very remarkable that it should have appeared on one of the critical days—upon the day when, in the ordinary course of fever, the disease should have subsided. And it strengthens greatly the opinion, that not only the general disease is under the law of periodicity, but that the secondary alterations are so too.

As a general rule, gentlemen, in prognosis, the occurrence of any vesicular eruption whatsoever in fever is bad. We have in fever several forms of vesicular eruptions. But, suppose a patient in the eighth or tenth day of fever, when he is otherwise going on well, you take his hand to feel his pulse, and you are surprised at seeing a vesicle upon his arm. Do not neglect that—do not overlook it. The mere circumstance of that solitary vesicle forming so silently, without any pain, without any notice, points out that there is mischief before you—that the case is likely to go wrong.

It is a very important prognostic. Here we have on this patient an eruption of vesicles running rapidly to deep gangrenous destruction of the part, for some of the cavities formed by them are singularly deep, though circumscribed. Now, suppose that in place of that disease attacking the skin, upon the fourteenth day, on the tenth day, or whatever day you please, in any case, a similar condition should attack the intestine. Suppose that the typhoid matter suddenly infiltrated a gland in the intestine, and that that should fall as suddenly to gangrene, and that there was a solution of continuity; you will at once see the progress of the worst form of the typhoid secondary disease of the intestine. You may look on this man as at this moment turned inside out, and that on the surface of his skin you are able to learn the history of the worst form of the typhoid ulceration of the intestine. This extraordinary condition of his surface no one will say is inflammation of the intestine? Certainly not. Whether this man is to live, gentlemen, or to die, I believe none of us can venture to say. Of course, the chances are enormously against him; but, in the treatment of fever, you are never to despair so long as the patient can swallow. So long as he is able to take nourishment, so long as he is able to swallow wine, no matter how dreadful or apparently hopeless the symptoms may be, you are not to desert him, but—to use the phrase of our glorious sailors—you are to fight the ship while she swims. In a disease under the mysterious law of periodicity, every hour of compelled life is a clear gain. And, over and over again, you will find that your efforts will be crowned with success. You will see a patient lying with his back icy cold; you will see him pulseless,—his lungs filled up with secretion,—his belly tympanitic,—with dreadful diarrhœa,—the lower extremities gangrenous,—himself in a state of insensibility,—and yet, even under those circumstances, a recovery is possible. But that recovery can only be effected by the steadfast determination of the physician not to desert his post until the vital spark has actually fled; and, if you commit an error in holding on,—in hoping against hope,—at all events it is an error on the right side.

Lecture V.—With reference to the case of Hardcastle, with which we were occupied at our last lecture, we may hope that the symptoms have at last yielded. The great interest of this case consists in its having been an example of a fever in which the patient was scarcely maculated yet in which the stimulating treatment had to be pursued with an activity as great or greater than that which we are called on to employ in the worst cases of spotted typhus.

On looking over the notes of the case, I find that on his admission, which was on or about the seventh day, he had a few scat-

tered maculæ on the abdomen, of a large size, and of a leaden gray color. He then had diarrhœa, abdominal tenderness, and ileo-cœcal gurgling; and the sounds of the heart, though weak, preserved their natural mutual relations as to force and duration. Doubtless the patient at this period might have been well described as laboring under typhoid and not typhus fever, according to the distinctions now in vogue; but on the fourteenth, or the twenty-first, or the twenty-fourth day, he would be a bold man who would declare that the case was not typhus of the worst description. He is now at the twenty-eighth or thirtieth day of his illness, and we have every reason to believe that all will now go well. Since the eruption of gangrenous vesicles of pustules, which occurred on the twenty-first day, there have been no new appearances of this form of disease, there has been no new bedsore, nor have the gangrenous patches spread as might have been expected; two of them are in the form of sinuous cavities, but even these show signs of healing. These sores on the back were treated first, you will remember, by the simple and afterwards by the fermenting poultice; but as the latter gave considerable pain, the nurse returned to the simple poulticing, and now, under the advice of my colleague, Mr. Smyly, we have changed our plan, and are using stimulating dressings to the ulcerated surfaces. In the management of these sores, whether they be the ordinary bed-sores or examples of the gangrenous pustules, both of which appear to be of the nature of the genuine secondary affections of typhus under the law of periodicity, it sometimes happens that we have to deal with extensive sinuses. I have seen them sometimes of not less than seven inches in length. In most cases, the best treatment for them will be stimulating injections, using as an apparatus the vulcanized india-rubber bottle with a long and slender ivory pipe; and it is generally requisite at first to wash out the sinus with tepid water, and afterwards to inject some of the metallic solutions—diluted solutions of sulphate of copper, nitrate of silver, or sulphate of zinc—and when the discharge is very fetid, you may use the decoction of bark, or the solutions of chloride of lime or soda. An excellent dressing when the sore is open is the Canada balsam combined with oil, or a mixture of equal parts of castor-oil and balsam of copaiba. You will derive advantage, also, from the employment of pressure by means of flat compresses of lint, and a roller when it is possible to apply it, or in other cases you may employ strapping with adhesive plaster. When the sinus is in a depending position, and matter accumulates in its lower portions, it is sometimes necessary to make a counter opening; but this operation should rather be delayed until the system of the patient has improved; the case is then to be treated as an ordinary surgical case.

Now let me draw your attention to the diligence with which

the administration of wine has been pursued in this case. We began the exhibition of wine on the 2nd of November, which was the eighth day of his disease; and, for the next four days, the quantity administered was six ounces daily. The wine used all through was port, of an excellent quality. From the 6th to the 11th he had twelve ounces daily, and from the 11th to the 18th his daily allowance was twenty-four ounces. During the next three days it was reduced to eighteen ounces; and, from the 21st to the 26th, to twelve ounces per diem. On the 26th and succeeding days he had ten ounces; on the 28th the quantity was reduced to eight; on the 4th of December he had but four ounces; and the wine was omitted altogether on the 5th. Besides all this, a tumbler of hot brandy punch, containing two ounces of the best brandy, was administered from time to time whenever the patient's state seemed to require it; and in this way he used about twenty-four ounces of brandy, and four hundred and thirty-eight ounces of wine, and, in addition, bark, musk, and ammonia were freely exhibited.

Some may suppose that this quantity of wine was excessive. Such is not my opinion, nor will it be yours when you come to treat the typhus of this country in a large hospital. I am sure we might have given him with safety much more, and I doubt if we could have done with less. And the result of the case confirms what I have said to you before, that in a case of fever, no matter how terrible the group of symptoms may be, you are not to despair so long as the patient can swallow. Bear in mind, too, that all through, Hardcastle has been carefully fed with chicken broth, beef-tea, and jelly; and we have now passed that period when, under the law of periodicity, his terrible disease will subside.

This case brings to my recollection two remarkable instances in which an extremely bold use of stimulants was the means of saving life. Both were cases of most malignant typhus, and in both instances the patients were medical men. In the first, we administered more than two dozen of wine, of which six bottles were champagne; and besides this, the patient had not less than six bottles of brandy. His fever did not pass the twenty-first day; it was, therefore, at least ten days shorter than that of Hardcastle's, so that the stimulants were not only in greater quantity, but exhibited within shorter intervals of time. The symptoms in this case were the most extreme adynamia, continued coma, persistent boldness of the breath, enormous vibices, and a general purplish black color of the skin. We could discover no local lesion; the recovery was perfect, and the gentleman has now remained for many years in excellent health. He was a man of very temperate habits. In the second case, on about the twelfth day of the fever, I was called to see the patient; he had got no stimulants. The nervous system was free, the surface was cold, and the pulse

almost imperceptible; the integuments of the lower extremities, from the middle of the calf downwards, were black, and hung in loose wrinkles on the limbs. This patient swallowed about sixteen ounces of brandy in the course of a few hours; reaction was established, and the case did well. We expected extensive mortification in the legs, but within four-and-twenty hours from the commencement of the exhibition of stimulants we found that the circulation was fully re-established, the extensive blackness had disappeared, the limbs had recovered their fulness, and the subsequent loss of integument, which occurred only in the feet, was very trifling. In this case there was a petechial eruption, approaching to purpura; but although it is generally true that stimulants are more often called for in the maculated than in the non-maculated cases of fever in this country, yet you must bear in mind, that as in the case of Hardcastle, you will meet with a typhoid fever, if you choose to call it so, in which the most energetic stimulation is required. I have given an example of this in my paper on the state of the heart in typhus fever, in vol. xv. of the *Dublin Medical Journal* (first series). The patient used 292 ounces of wine, 20 ounces of brandy, 7 bottles of porter, and had besides various medicinal stimuli and abundance of food. The principal symptoms were extraordinary prostration, coldness of the surface, and feebleness and irregularity of the heart's action; and it was not until the end of the eighth day of the exhibition of stimulants in great quantities that any favorable influence was produced in the circulation; and the case strongly illustrates the advantage of persisting in the use of stimulants, although for days together no amendment seems to follow their employment.

I have sometimes observed that students were under a misapprehension about the doctrines which we have long held in the hospital with respect to the condition of the heart as a guide for the use of wine. They have come to the erroneous opinion that we are only to give wine where we find the want of the first sound of the heart, and that we are not to give wine where the heart is acting well. This is a mistaken view of the matter. What we have established as to the state of the heart in connection with the effect of stimulants, is simply this,—we have ascertained that the efficacy of stimulants is often directly as the debility of the heart. It has been also ascertained that the power of bearing stimulants, their effect upon the nervous system, their good effects on the general condition, are directly as the weakness of the heart. We may lay down as a rule, that there are three conditions of the heart to be looked at by the practical man in the treatment of fever. In one we have an excited heart—a violently excited heart all through the case; and this heart may be excited and violent, although the symptoms be those of extreme adynamia, although the surface be cold, the breath cold, and the pulse so feeble that

it cannot be discovered. Nay, the heart may act with great force for several days, and yet there be no pulse at the wrist. This is one case. In the next case, we find exactly an opposite condition, in which the systolic force of the heart is diminished. This is shown by loss of impulse of the heart, by diminution of the first sound, and, in certain cases, by extinction of the first sound of the heart while the second remains. This is a case which calls for wine, and in which you should give it; it is a case in which in the vast majority of instances, wine will agree with the patient. There is a third set of cases in which the heart does not seem to be implicated at all in the course of the disease, in which, notwithstanding the existence of the most extraordinary group of symptoms affecting various organs, the heart, in the middle of the storm, seems to be in a state of calm and quiet. If we compare these three sets of causes with a view to prognosis, we may arrange them in this way. The case of excited heart all through, with feeble pulse and with adynamia, is unquestionably the worst case. There is no worse symptom in fever than an excited heart. It is especially a bad symptom when, with that excitement, we find a feeble pulse. The next will be the case of sinking of the heart; and the most favorable case is that in which, as I said before, the heart seems to escape disease. But you are not to suppose, that because you have an excited heart you are not to give wine if the symptoms of the patient require it; and you are not to suppose that, because the heart is not affected at all, you are to withhold wine if the general symptoms of the patient require it. You are not to found your exhibition of wine or stimulants upon any one thing; you are to take the general state of the patient into consideration. What we have done, is to discover an intelligible practical rule which will guide you in the use of wine in certain, I think in many cases; but you are not to suppose that because this man has a clear first sound at his heart, therefore you are not to give wine. You are not to suppose that because the heart is safe you can do without wine. Now, in the case of Hardcastle, recollect that although his heart seemed to escape, or was at most only feeble through the course of the disease, what frightful adynamia existed; how for day after day the patient's face was hippocratic, or almost so; how the general character of the disease was that of the most terrible putrescent fever; yet his heart escaped. And here is the result. We have given that man upwards of twenty bottles of wine and twenty-four ounces of brandy and now, on the twenty-eighth or thirtieth day of the disease, we have the satisfaction of feeling that his case may be set down as among the triumphs of medicine. I wish also strongly to impress on you the great importance of the use of other forms of nourishment in this disease; for we must not only keep up the nervous energy of the system by wine, but we must support nature by

food. There is no mistake more fatal in fever than the withholding of food. I was early taught the importance of the use of careful nourishment in fever by my friend and colleague, Dr. Graves. I remember once, Dr. Graves, when speaking of the necessity of the use of nourishment in fever, made use of these words,—"If you are at a loss for an epitaph to be placed on my tomb, here is one for you,—'*He fed fevers.*'" In addition to the prejudices with which the inflammatory doctrine imbued so many minds, with respect to the use of food in fever, there was a new set of arguments raised against it, in consequence of the experiments of an American physician. I allude to the case observed by Dr. Beaumont, and so often quoted since. In this remarkable case, various medicinal substances and articles of food were introduced through an external fistula into the stomach, their effects being noted, as also the conditions of temperature, vascularity, etc. A set of results were subsequently published in connection with the action of the stomach upon food. One of the results stated to have been thus obtained was, that the existence of the state of fever altogether suspended the process of digestion. Here was a statement which had the appearance of being the result of strict observation. It influenced a number of young men; but did it influence those who had once been in charge of a fever hospital? Not at all; because those men knew very well that, no matter what Beaumont might say about the stomach not digesting when the patient had fever, in thousands of cases patients in fever digested remarkably well, required food, and derived benefit from it. In a large number of cases of typhus fever, the stomach has an excellent power of digestion; and, I believe, if we were bold enough, we would find that many articles of food usually forbidden to fever patients might be given to them with safety. A curious incident was related to me which shows that the stomach in fever is capable of digesting even a rather coarse article of food. A lady who had been recently married was attacked with extremely severe petechial fever; she was covered with dark-colored maculæ, and the disease had run to about the twelfth or thirteenth day. She was attended by several eminent physicians. Her case was an extremely bad one, and her life was all but despaired of. She was violently delirious. Her husband had occasion to leave the house on some business. At the period of the dinner hour of the family, the servants were cooking a rump of beef and cabbage, and the odour of it filled the house. In her delirium, she called for some of the beef and cabbage; she was then, you must understand, in severe fever, and covered with maculæ. Her sister, who was then attending her, believing she was dying, thought it only right to indulge her, from the feeling that it was right to indulge the request of a dying person. She proceeded to the kitchen, and as soon as the beef was boiled, cut a very large mass of beef and

cabbage; and this was brought up smoking hot to the lady's bedside, when she devoured it with great avidity. Shortly afterwards her husband came in, and was told what had happened. He became terrified, and sent for physicians in every direction. Four or five assembled; time was pressing, and every one agreed that something should be done. At length the late Dr. Harvey, a practical physician of the very first class, arrived. He was laid hold of by the agonised husband, forced up stairs, and his opinion earnestly requested. At that time, the stomach pump was not in fashion, but every one agreed that something decisive should be done, that an emetic should be given, or some extraordinary effort made to get this mess of beef and cabbage out of the lady's stomach. When Dr. Harvey went to the bedside, he found the patient in a tranquil sleep. He turned round, and when anxiously appealed to what should be done, he said—"You had better wait till she wakens; let her sleep it out." She slept for four or five hours; awoke wonderfully better, and on the following day was out of danger. I do not give you this case to induce you to feed your patients with salt beef and cabbage in fever; but it is very important, as showing that in typhus fever, with maculæ, the stomach is capable not only of digesting such a coarse article of food as salt beef, but that even such food may have a good effect.

It will be remembered by the readers of the *Western Journal of Medicine*, that Prof. Flint and some other medical gentlemen of Buffalo detected the Misses Fox, a few years since, in the act of producing *Spirit-rapping* with their knees. A similar case is given in the following extract from the *London Lancet*, for September:—

Disclosures respecting Spirit-Rapping.—At the meeting of the Academy of Sciences of Paris, on the 12th of June last, M. Rayer called the attention of the members to some experiments of Dr. Schiff, of Frankfort, touching spirit-rapping. This physician had under his observation a young girl, with whom certain noises took place, attributed to the rapping of spirits. Dr. Schiff, by paying much attention to the case, found out that the sound issued from the girl herself, and not from any other source. The noise is, in fact, produced by the slipping of the tendon of the peronæus longus from the sheath in which it glides in passing behind the outer ankle. Dr. Schiff succeeded in repeating upon himself the same phenomena presented by the girl, which was supposed to depend upon a rapping spirit. When the sheath of the tendon is weak and lax the noise is very easily made, and may be produced with-

out moving the foot in the least; but when the finger is placed behind the outer ankle just when the sound is being made, the alternate and repeated displacement is felt without difficulty, the action being accompanied by a rapid up and down movement of the same tendon. The Secretary of the Academy requested Dr. Schiff, who was present at the meeting, to repeat the experiment before the members. The rapping was accordingly produced, and heard at the distance of several yards, though silence was far from being complete. The feet were quite open to view, but no movement could be perceived.

On the External Application of Belladonna in Delirium Tremens. By Dr. Grieve, Physician to the Dumfries and Galloway Infirmary.—Reflecting upon the contracted state of the pupil in the second or developed stage of delirium tremens, Dr. Blake imagined that, "by dilating the pupil we might so influence the disturbed visual sense as to dispel, or at least modify, those 'false creations proceeding from a heat oppressed brain' which characterize this disease, and thus conduce to the comfort and tranquility of the patient;" and one case is related in which this plan was adopted with apparent benefit. In support of his idea, Dr. Grieve reminds us that the late Dr. Graves proposed the use of belladonna in such cases of fever as were attended with cerebral disease, and contraction of the pupil.

Case.—On the 25th of March last I was called to attend D. W., aged forty-nine, a man naturally of a robust constitution, but who, of late years, had been much given to intemperance. On inquiry I found that he had been more or less intoxicated for the last three weeks, that he had slept none for several nights in succession, and that the present was his fourth attack of delirium tremens. I found him suffering under great nervous excitement and commotion; laboring under all sorts of optical delusions; fancying that lizards, centipedes, and other entomological horrors were crawling in and around his bed, from which he was convulsively making vain efforts to dislodge them. His pulse was upwards of 120, soft and compressible; his whole body was bedewed with a cold clammy perspiration, and the pupils of both eyes were much contracted. Having obtained some extract of belladonna, I rubbed a little on the eyelids, and remained by his bedside to mark the result. My expectations were soon more than realized, for no sooner was the physiological effect of the drug manifested in the dilated state of the pupils, than the spectral illusions gradually became less and less distinct, the nervous tremors and excitement began to subside, and he soon became comparatively quiescent and tranquil. Soon after this I had the satisfaction to see him fall into the much

coveted sleep. Thus I left him; and on revisiting him in a few hours I found that he had slept for two hours; his pupils were then still much dilated; his pulse was below 100, firmer, fuller, and of better character; and altogether his condition, mental and corporeal, was much ameliorated. On interrogating him about his recent hallucinations, he replied, "They were all stuff and nonsense; I see no more of them."—*Edinburgh Monthly Journal*, Nov., 1853.

Report of M. Giraldes on the Employment of the Per-chloride of Iron.—M. Giraldes has published, in an interesting *Mémoire*, an account of thirty-five experiments by the injection of the perchloride of iron into the arteries of animals. These experiments were commenced in the month of April, 1853, and continued to the month of March, 1854. They prove that a few drops of the perchloride of iron, of variable density, injected into the carotid artery of a horse, coagulate the blood contained in a portion of the vessel somewhat less than an inch in length. Thus, two drops at 49° areometer of Beaumé, three drops at 30°, and six drops at 15° produce this coagulation. The changes produced upon the blood and upon the walls of the vessel by the coagulating agent vary, *cæteris paribus*, according to the density of the perchloride. Five drops at 49° mummify (*momifient*) completely the blood contained in the artery. The same quantity of the perchloride at 15° to 20° produces a sort of clot formed by blood combined with the salt of iron and by normal fibrine. But the action of the coagulating liquid is not completely exhausted upon blood; it extends to the coats of the artery. In the first case, these membranes become disorganized; they present a yellowish-brown discoloration; become thinned, horny, and in fact mortified. In the second case the membranes of the artery are modified by the action of the chemical agent, but this modification is much less intense, and does not amount to disorganization. The epithelium and the internal coat are destroyed; the middle coat having lost its contractile properties, yields to the impulse of the liquid, and dilates; its circular fibres are easily seen upon the inner surface of the vessel. This condition is accompanied by yellowish discoloration of the fibres of the middle coat, and by their adherence to the clot. It may be asserted, that the injection of the perchloride of iron into an artery is always followed by these modifications:—

1st. The formation of a clot.

2d. Modifications in the organization of the arterial tunics.

The clot offers different characters, according as produced by an injection of the perchloride at 45°, 49°, or 30°. In the first

case it it compact, homogeneous, and formed in totality of altered blood; in the second, it is formed by a mixture of blood, altered by the salt of iron enveloped in a large quantity of normal fibrine. These primary clots are followed by the formation of two others, one on the side of the heart, the other towards the periphery.

The coats of the artery present very important modifications in their organization; whenever the experiment is made with perchloride at 49°, they become disorganized; they are reduced to the state of foreign bodies, and require to be cast off. But if the experiment is made with perchloride of 15° to 30°, the changes are of quite another character. The middle coat becomes hypertrophied, contracts adhesions with the clot, which it tends to encyst in the cavity of the vessel. The external coat is infiltrated by a fibrinous matter, plastic lymph, which may extend for some distance. After the first formations, there is established in the diseased structures a process of elimination and repair. The process of elimination takes place when both artery and clot are disorganized; they soften, and are cast off. These changes may extend to some distance, when the feeble connexion between the coats of the vessel and the secondary clots gives way, and fatal hæmorrhage ensues. The work of repair is established when both clot and arterial tunics are completely organized. The clot may soften without decomposing. But when the process of repair goes on favorably, it becomes encysted in the vessel, adheres intimately to the artery, and obliterates its calibre. As these phenomena proceed, both the plastic formations and the secondary clots disappear.

The injection of the perchloride of iron into an artery may, therefore, give rise to two classes of phenomena, primary and secondary.

The primary phenomena are:—

1. The formation of primary and secondary clots.
2. The infiltration of plastic lymph into the sheath of the artery and adherence of the clots.

The secondary phenomena are:—

1. The elimination of the disorganized parts.
2. Hypertrophy of the middle coat.
3. The encysting of the clots.
4. The disappearance of the secondary clots and of the plastic formations.
5. The occlusion of the artery.

The conclusions to which Dr. Giraldés arrives are:—

1. The perchloride of 49° to 45° should not be employed either in aneurisms or erectile tumors; its use may be followed by serious accidents.

2. In aneurisms and in erectile tumors, both venous and arterial, the perchloride should be either of 30° or 20° areometer of

Baumè in the proportion of 5 drops of 30°, 10 drops of 20°, for a quantity of blood equal to 3 cubic centimetres.

3. The perchloride of 45° to 46° may by used as a hæmostatic to stop deep hæmorrhages, or secondary hæmorrhages, after operations.

4. The perchloride of 25°, 20°, or 30° may be advantageously employed in hæmatic cysts, especially when they occur in the neck.

5. In certain cases, the perchloride of 30° to 49° may be employed to modify the condition of wounds in suppuration.—*Gaz. des Hôpitaux.*

Protracted Pregnancy.—Dr. Edward Smith mentions a case in which it appeared clear to him that the woman was delivered in ten months after conception took place. He also mentioned a case in which a child cried so loudly, three minutes after the head was born, and before the body was extruded, that it could be heard in the adjoining room. This case showed that when a pelvis was large, and the pains not severe, there might be sufficient room for the undelivered chest of the child to expand. Had there been a question in this case as to whether the child was born alive, it could have been shown that it had breathed. Dr. Smith also related a case in which an infant, two days after birth, had two of the incisors fully developed in its gums. In such cases, did the permanent teeth come earlier than in ordinary cases, and when teeth were developed in utero, were there three sets of teeth?

Mr. Charles Clark mentioned two cases in which pregnancy appeared to him to have been prolonged in one instance to ten, in the other to eleven months.

Mr. W. A. Harrison remarked, that in cases where teeth were developed so early as in Dr. Smith's case, they fell out early, the gums remaining bare for some time before the permanent teeth presented themselves, but the child had not three sets of teeth.

Mr. I. B. Brown and Mr. Dendy remarked that it was not rare to observe cases in which children cried before the shoulders were delivered.—*Lancet.*

On the Comparative Mortality of Pestilence and War. By Dr. Robertson.—Some interesting tables have issued from the Health Office, comparing the loss of life by war and pestilence. It appears that in 22 years of war there were 19,796 killed, and 79,709 wounded, giving an anual average of 899 killed, and 3623 wounded. In 1848–49, there were no fewer than 72,180 persons

killed by cholera and diarrhœa in England and Wales, and 144, 360 attacked; 34,397 of the killed were able-bodied persons, capable of getting their own living! Besides these deaths from the great epidemic, 115,000 die annually, on an average, of preventible diseases; while 11,419 die by violence. Comparing the killed in nine great battles, including Waterloo, 4,740, with the number killed by cholera in London in 1848–49, 14,139, we find the difference of 9,399 in favor of war. In cholera visitations, twelve per cent., sometimes twenty per cent., of the medical men employed died. The London missionaries die as fast as those in foreign countries, and there are some districts in London which make the Mission Society ask themselves whether they have a right to send men into them. From the returns of twelve Unions it is found that 3,567 widows and orphans are chargeable to the cholera of 1848–49, entailing an expenditure of £121,000 in four years only. —*Edinburgh Monthly Journal*, May, 1854.

Treatment of Intermittent Fever by Quinidine. By William Pepper, M. D., Physician to the Pennsylvania Hospital. — In January, 1853, I published in the Amer. Jour. of Med. Sciences, a number of cases of intermittent fever, treated by the sulphate of cinchonia. From that time up to the present, I have used no other remedy in my hospital practice for this class of diseases, and the result has fully confirmed the conclusions then obtained. In April last, at the suggestion of Dr. Conrad, I was induced to try the recently discovered alkaloid, quinidine, in place of the cinchonia; unfortunately, however, the number of suitable cases of intermittent, admitted during the past spring, was unusually limited, amounting in all to only five cases, and entirely too few to establish any positive conclusion in therapeutics, and yet sufficient to invite the attention of the profession to a more extended trial of its virtues.

Before proceeding to report these cases, it may be well to state a few facts in regard to the remedy under consideration. As far back as 1833, Henry and Delondre discovered this alkaloid, and gave it its present name; the following year, however, further investigations induced them to believe that it was identical with quinine. In 1848, Winckler, a distinguished German chemist, gave a full description of it, and some of its salts; he at first, was also disposed to consider it as a mere hydrate of quinine; but by patient investigation, finally proved that it was a distinct substance, possessed of many distinct physical and chemical properties. In the last edition of Pereira's Materia Medica and Therapeutics for 1854, it is stated that the quinidine may be obtained from most of the genuine cinchona barks, by the same processes

that are used for procuring quinine; the sulphate of quinidine being more soluble than the same salt of quinine, the former is left in the mother waters. The most important fact, however, in connection with this subject is, that this new alkaloid is found to abound in the cheaper kinds of barks from New Grenada; the Bogota cinchona, which contains but little quinine and a large amount of quinidine, is now largely used in England for obtaining this last named alkaloid. It can now be obtained at Powers & Weightman's Manufacturing Chemists of our city, at fifty cents an ounce less than quinine; and there is no doubt that it could be supplied by the importation of the cheaper kinds of bark, at a price not exceeding that of cinchona itself.

The sulphate of quinidine used at the hospital, presented much the same appearance as the salt of quinine, nor was there any marked difference in their taste or solubility. For those who desire to investigate more fully, the chemical and physical properties of this substance, I would refer to an elaborate paper by Winckler, in the Pharmaceutical Journal for 1848, vol. vii, p. 527; as also to an article by the same author, in Chemical Gazette, vol. vi, p. 164. No allusion, however, is here made to its therapeutic properties, nor have I been able to refer to any author, where such information might be obtained. Mr. Procter, Ed. Journal of Pharmacy, informed me that he had met with the same difficulty, and that with the view of elucidating this all important point, he had quite recently requested a medical friend to test its powers in intermittent fever; the result, he states, was most satisfactory in the only four cases of obstinate disease in which it was tried. In reporting the following five additional cases, I shall be as brief as possible.

Case 1st.—A laborer, æt. 26 years, entered the hospital April 5th, with intermittent fever of the tertian form, the paroxysm generally coming on at 10 A. M.; he had been suffering with the disease for two weeks. On the 6th, he had a severe chill, lasting thirty minutes, and followed by the usual hot and sweating stages. Accordingly on the morning of the 8th, he was directed quinidine sulph. grs. x, grs. ij. every hour, commencing at 5 o'clock.

From this time the patient had no return of the disease, and although he remained in the institution until the middle of June, had no relapse during that time.

Case 2d.—T. W., laborer, admitted May 27th, stated that he had been subject to chills and fever for four months, and that he had contracted the disease in Savannah. At first, the paroxysms appeared to have come on about 10 o'clock, every other day; lately, however, they had assumed the quotidian type, but still occurred at the same period of the day.

Although the patient had a chill on the day of his admission, as also on the following day, no treatment was instituted until the

29th, when he took the quinidine sulph. grs. ij. every hour, commencing at 8 A. M., or just five hours before the expected paroxysm. The only perceptible effect in this instance, however, was the mitigation of the chill, and its postponement for about one hour. On the following day, the same treatment was assumed, and with the most perfect success; he remained in the house about ten days after this, but during his stay had no relapse.

Case 3d.—A young Irish woman, aged 22 years, was admitted June 2d.; she stated that she had been suffering all last Fall with chills and fever, and that she was finally cured of her disease, after entering this hospital. Since then, she has remained perfectly well up to May 20th, when the chills again appeared, and continued to recur daily at about 10 A. M.

With the view of ascertaining the character of the attacks, it was deemed most expedient not to interfere until the 5th, or three days after admission. She now took quinidine sulph. grs ij. every hour, commencing at 5 A. M., until in all grs. x, had been taken; without, however, checking the paroxysm, though it was certainly considerably mitigated.

On the following day, June 6th, the same plan was pursued, and with the effect of completely checking the disease; which, in fact, did not return during the ten days she remained in the hospital.

Case 4th.—A laborer, aged 48 years, entered the house June 13th, suffering with a severe chill; he had the same on the five previous days, the paroxysms coming on in the course of the forenoon, but rather irregularly. On the 14th, he had a chill, lasting nearly two hours, and commencing about 10 o'clock, A. M.; on the following day, he took the quinidine grs. x, as in the former instance, grs. ij. every hour in anticipation of the chill. It, however, came on at the usual time, but was exceedingly mild and lasted only thirty minutes.

On the 16th, the remedy was administered in like manner; upon this occasion it effectually checked the disease, nor was there any relapse up to July 1st, when he left the Institution. In this case, it should be mentioned, that as the patient was somewhat anæmic, and had enlargement of the spleen, gr. i. of the quinidine combined with gr. v. of carb. Ferri, (Vallet,) was continued three times a day, during his stay in the house.

Case 5th.—A sailor from Mobile, aged 22, entered June 17th, sick two weeks with intermittent fever; the attacks coming on daily at about mid-day. The patient was somewhat prostrate and slightly jaundiced.

The treatment by quinidine was commenced on the 19th; the paroxysms, however, came on one hour earlier than upon the previous day, and the chill was quite as severe as usual. On the 20th, he again took grs. ij, for five consecutive hours in anticipa-

tion of a return; but from this time until he left the hospital, June 27th, he remained perfectly well.

Except in the first case above reported, the remedy had to be repeated a second time, before the fever could be fully arrested, but in no instance was its further use necessary. It is to be regretted that the cases could not have been retained longer under observation, so as to have fully ascertained as to the permanency of the cure; but the crowded state of the wards, and the unwillingness of the patients to remain when they felt perfectly well, render it impossible to effect this desirable end. It will be perceived that grs. x. was the largest amount given upon any one day; and as I have generally, under apparently similar circumstances, been obliged to give grs. xv. of sulph. quinia or cinchona, I am disposed to believe that the quinidine is more active than either of these alkaloids. In England it is manufactured chiefly with the view of adultering quinine; but if the above conclusion be confirmed by further observation, our patients' health will not suffer by such admixture, however unfavorably it may operate upon them in pecuniary point of view.

In conclusion, I would remark that Dr. Darrach, to whom I am much indebted for notes on the above cases, is anxious to continue the use of this remedy in all suitable cases that may occur during his residence in the hospital, so as to enable him, at some future period, to make a more ample report upon this subject.—*Medical Examiner.*

Hints for Young Doctors. By C. D. Griswold, M. D., of New York.—For ten years I have led a somewhat variable and busy life, always devoted to the interests of my patients—when I had them to care for—and my profession: yet notwithstanding my predelictions for the use of the pen, I have seldom contributed anything of my experience, or inexperience, to strictly professional journals; preferring always to read for my own instruction, rather than to write for the information of others. The principle is wrong, although it is better to be silent than to affect to be overwise.

How much more attentively we watch the different phases and behaviour of a disease, when it is our intention to report it—how much more definitely each symptom is impressed upon the memory; and with what readiness its stages and the treatment may be recalled at any time afterwards. In this way a habit becomes confirmed, and holds good in all cases. In traveling formerly, I noticed everything for the purpose of giving a description. Nauvoo Temple has long since been crumbled to the earth; yet the peculiarities of that structure, and the ground and beautiful scene

I looked out upon from its tower, are still distinctly visible in my recollection. The habit of observation thus formed, has led me ever since to the upper deck of a steamboat, or the top of a stage coach, that I might look out; and to detest cars, because they shut me up.

There is more *utility* in this habit of close observation, than most physicians are inclined to acknowledge in practice. I can remember distinctly the details of cases that I attended years ago, with the modifications in the treatment to meet indications—I held in view, and do still, intend to publish them—and yet I have by no means a retentive memory upon general subjects.

This habit any one can acquire by proper discipline, and it is one exceedingly important to the physician. The young physician who writes out his cases, will be most sure to read the reports of others, and in this way his experience will be trebled in value, besides most likely escape that worst of all obstacles to progress, routine habits of practice.

In reading your Journal, which I always do with interest, I seldom pass over the report of a case; and if I know it to be from the pen of a young physician, I peruse it with a sort of "do by others as you would that they should do by you" principle—respect and encourage them by reading their productions, as we would cherish the memory of one departed—although both alike are obvious to our good intentions. In this way we not unfrequently fall in with good ideas, which like seed sown, spring up at a future time and multiply.

As I shall have no room in this for a "report," as I had intended, I will add one other hint for such of your readers as may be younger than myself, and put off the "case" to another day—or rather night.

In the first place, write out all your important cases; if time will not admit its being done immediately, keep thinking them over with that intention. When this is done, if you find any of them to contain facts which you believe to be of value, send them to a publisher, post paid. Do not make the mistake that many young writers do, by sending to the largest and most important Journals, for such are usually supplied with more matter than they can print; and therefore, in all probability, in such a case your production would never find a place in their pages, and you would most likely get discouraged with the first attempt. On the contrary, send your articles to a small Journal first, or to a new one that has little patronage—of which there an abundance thankful for small favors; and in order that you may be sure to see them if printed, it is a good plan to enclose the subscription price with the production, and but very few if any comments, aside from your name and address plainly written. Draw no inferences on your cases—your readers will do that, and save you your time,

and paper, and likely enough no small amount of future regrets—but simply the medical facts, plainly and concisely stated. Remember, if you have any desire to see your article reprinted in other Journals, that it never will be if *long*. Follow these rules perseveringly, and you will ultimately not only succeed as authors, but as good physicians.

A *regular* medical man told me, not long ago, that he subscribed for but one Journal—and that I will not mention—which he never found time to read. Now I shall remember this man as long as I do the Nauvoo Temple, for I have a habit of remembering such "cases." I shall never apprehend, on meeting him, that he has seen this comment, for did he read this Journal he would know better than to make such a statement.

I remember calling on Dr. John W. Francis, late one evening, and finding him in bed, he not being very well; yet his light was safely arranged, and within reach there was reading matter enough to last all night. If there is no other time, an hour may be spent every night in reading, before the eyelids drop; and he who cultivates his intelligence, as a physician should, will improve even this hour, if he has no other. If you do not read, never tell of it, for it is more creditable by far to have time for this, than too much business.—*Boston Med. and Surg. Journal.*

Ergot: Circumstances which contra-indicate its use during labor; by M. Paul Dubois, M. D.—Ergot, besides its well known hemostatic properties, is useful when, from inertia, the uterine contractions are insufficient to complete delivery. It is, however, to be regreted that this valuable remedy is so often improperly used. We have frequently known it given when it was entirely unavailing, because unsuited to the case. This abuse, and the sad effects consequent upon it, have contributed very much to discredit ergot in the minds of many practitioners. We should, nevertheless, not be deprived of so precious an agent from the mere fact that it has been injudiciously administered. We should rather endeavor to ascertain the circumstances under which it may be advantageously prescribed, and distinguish these from the contraindications to its use. The circumstances under which the uterine contractions may be impaired without calling for the use of ergot are sufficiently numerous to merit attention. The following are some of the most common:

1st. Debility dependent upon a natural state, or induced by previous disease. This requires tonics, broth, wine, &c., for the purpose of improving the strength of the patient during labor.

2d. Extreme distension of the uterus from an excessive quantity of liquor amnii, which induces incomplete paralysis. The

uterine fibers being inordinately distended, the membranes should be punctured whenever the os tincæ is one-fourth dilated, or about the size of a half dollar.

3d. Congestion of the face, attended with impaired contractions from plethora. The patient should then be bled.

4th. Mental disturbance from whatever cause, sometimes from the presence of particular persons in the chamber, may suspend the uterine contractions. The woman must be quieted and the causes of annoyance removed.

5th. Great elevation of temperature in the apartment may lessen the contractions by the cerebral congestion it induces. The room should, in such cases, be well ventilated and cooled if possible.

6th. Pain other than that of urine. Retention of urine, may be so painful as to impair the contractions; catheterism should then be practiced. Very great pain in the loins may likewise affect them, in which case we should apply the forceps if labor is sufficiently advanced; if not, wait patiently. The same may be said with regard to the headache which sometimes attends each abdominal contraction.

7th. The premature discharge of the waters makes labor slow, because, like all hollow organs, the uterus contracts most forcibly upon unyielding contents, and that when the membranes are ruptured the descent of the fœtus at each contraction lessens its force.

8th. The uterine contractility may become exhausted by the tardy rupture of the membranes in consequence of the firmness of these; rupture them. Rigidity of the os tincæ from phlogosis may do the same; resort to belladonna or to general bleeding. If the cause be unyielding induration of the tissues of the cervix uteri, of which we have recently seen a case, the knife should relieve the resistance.

9th. There may be posterior obliquity of the cervix which will lessen the force of the contractions. If the os does not dilate of itself (and cannot be rectified by the finger—Ed.) we may have to divide its anterior lip with a probe pointed bistoury.—*Southern Med. & Sur. Journal.*

Anasarca, treated by decoction of Onions, cured. By Ramon Uranza, of Navarre.—Manual Goñy, native of Berriozar, aged 16, shepherd by profession; lymphatic temperament, constitution feeble, idiosyncrasy gastro-hepatic. Has enjoyed good health until a year since, when he was attacked with a pulmonary catrrh, which confined him to bed for some days. He recovered sufficiently to resume his ordinary pursuits, though frequently

troubled with cough, until the beginning of December, when his condition was described as follows:

Decubitus on left side, frequent and full pulse, heat of skin, intense thirst, cephalalgia; tongue red on the edges and tip, furred in the centre; anorexia, frothy urine, frequent dry cough and sense of pain through the whole chest.

Diagnosis.—Chronic pulmonary catarrh, with intense exacerbation in consequence of exposure to cold.

Treatment.—Venesection, barley water for diet and drink. The bleeding was repeated the next day, and a pectoral ptysan prescribed. Under the use of demulcents for six days; symptoms abated and expectoration was less painful.

December 12th, 8th day.—Remarked that the belly and inferior extremities had increased in volume, the fever continued, and the urinary secretion was very scanty, and the thirst very intense. Diuretics and sudorifics for four days with advantage; but on the 12th day of the case, anasarca was manifest, not only in the belly and lower extremities, but also in the upper, as well as in the face and neck.

The rapid progress of the disease, destroyed not only my confidence in the resources of science, but also that of the patient and his friends; he was left in the hands of the priest to afford him the consolations of our holy religion.

On the 16th day, the young shepherd was threatened with asphyxia, exciting the terror and admiration of beholders, on account of the enormous distention of the skin, which hindered all motion.

In this critical and dangerous condition, I remembered to have read in "El Porvenir Medico," a case of ascites in Almaden, reported by Juan Francisco Gallego, cured by onions and milk. My patient feeling no disposition to drink milk or eat raw onions, owing to dyspnœa, I directed a decoction of two onions of ordinary size, in a gallon of water, to be drank *ad libitum*.

Though no relief was found on the second day, the decoction was continued, and in three days, (20th day of the case,) the thirst was diminished, the flow of urine had increased, and the skin of the lower extremities was somewhat wrinkled.

On the 23d day, the abnormal volume of the belly and extremities was reduced one half, so that the patient could assume any position in bed. He had some appetite and was allowed broth. Decoction of onions continued.

On 33d day, (25th of the anasacra,) he was without fever, had a fair appetite, and no serious infiltration could be discovered. The decoction was continued, and at the end of a few days, he returned to his habitual occupation.

I should remark that I have since treated a young woman, who

after a severe and long continued disease, was attacked with anasacra, with decoction of onions. In a very few days, copious diuresis occurred, and convalescence was in a short time established. But I refrain from reporting the details of this case, because other remedies were employed at the same time.—*El Porvenir Medico.* Madrid, March 15th, 1854.

Influence of Variola and Vaccinia. By W. Cooper, Esq., M. R. C. S. E.—Through the kindness of Mr. Skegg, of St. Martin's-place, I have seen, in the Strand, a case completely illustrative of the mutual influence of variola and vaccinia.

A girl, exhibiting no vaccine cicatrix, had attended and slept in the same room with her sister, affected with confluent variola. During this exposure she was vaccinated by Mr. Skegg, on the 18th of May, with fresh lymph. Three punctures were perfect. On the 22nd, premonitory symptoms of variola were observed; and on the 26th, papulæ. This was the eleventh day from the vaccination. On the 30th, when I saw her there were three very large, round, flat, dull-yellow pustules at the points of vaccination; variola plentifully scattered over the body. On the 2nd of June, eighteen days from the punctures, and seven days from the variolous papulæ, the variolous pustules changed to a brown, filmy scale, rapidly falling off, nothing like maturation or secondary fever occurring.

The points both of curiosity and practical interest are, the synchronously modified or hybrid character of the two pocks, seeming, unless variola and vaccine be identical, to invalidate John Hunter's axiom; the antidotal or extreme mitigation of semi-confluent and extensive variola, although not absolutely prophylactic, and, above all, the total prevention of maturation and secondary fever. *A priori*, we should at once pronounce, on the seventh day of a perfect areolar and progressive vaccine vesicle, complete immunity from variola; nor should we be prepared to see, as in this case, vesicles converted into unilocular cysts containing discs like the matured variolous pustule. A slight revolution has taken place regarding Jenner's adaption. Such a case as this, however, should make the objector pause, as it proves the value of vaccine far more than cases of prophylaxis, which may often be merely negative. These cases are very rare, but I could refer to five or six others. My point, however, would not be thereby strengthened.—*London Lancet*, June, 1854.

Calomel and Quinine in Tropical Fever.—There cannot be a doubt that if not calomel, yet certainly salivation, is an antidote to malarious fever. The instant a patient's mouth is sore, the fever leaves him. The mercury produces not the slightest effect till then; but from that moment the disease vanishes as if charmed. The change is from death to life—from extremity of suffering to calm and comfort. Numerous instances, too, of the safety which salivation gives from the effects of the malarious poison, may be found in Dr. Johnson's book, such as patients salivated for syphilis sleeping with impunity in places which were fatal to every one of their companions; and also many cases are on record of officers in India passing in a state of salivation by Dâk, unharmed, through the most deadly jungles. But since the discovery of the invaluable quinine, we have wanted a bold return to the practice of our forefathers; for in quinine we have all the qualities required. It is not a bulky medicine, and does not cause vomiting. It is a perfect antidote, and will cure all cases of malarious fever not otherwise hopeless, and it has no bad effect, like mercury, on the constitution. The gradual introduction of quinine in place of calomel has left our mortality by fever much the same as in former days; but by the expectant treatment for malarious dysentery, where quinine has not been substituted for salivation, the statistics of the General Hospital for twenty-five years, show that the mortality is actually double now what it was under the salivating treatment.—Surgeon Hare *in Edinburg Medical and Surgical Journal.*

Gangrene of the Lung successfully treated by inhalations of Terebinthinate Vapours.—Dr. Skoda has published, in the *Zeitschrift*, &c. of Vienna, several cases of gangrene of the lung, in which the symptoms gave way by the use of terebinthinate vapours and the administration of quinine. In the first case, the cure was effected in six weeks upon a servant, with whom the gangrene had attacked the upper lobe on the right side. An innkeeper, of middle age, was equally benefited by the same means, but the cure took a longer time and a stay in the country; he also took one grain of quinine every second hour. The treatment was not properly carried out in the third case; and the fourth, that of a journeyman butcher, of a robust constitution, is still pending. The latter had, however, so far recovered, after using the inhalations, and also taking Fowler's solution, that he could go into the country, though there was still some uneasiness in the left scapular region. The inhalations are made by pouring oil of turpentine on boiling water, the inspirations being repeated every second hour, and carried on for fifteen minutes.—*London Lancet.*